PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXLVII.

PART I.

LONDON:

PRINTED BY RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET.

MDCCCXLVII.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions,* take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions,* that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

The Meteorological Journal hitherto kept by the Assistant Secretary at the Apartments of the Royal Society, by order of the President and Council, and published in the Philosophical Transactions, has been discontinued. The Government, on the recommendation of the President and Council, has established at the Royal Observatory at Greenwich, under the superintendence of the Astronomer Royal, a Magnetical and Meteorological Observatory, where observations are made on an extended scale, which are regularly published. These, which correspond with the grand scheme of observations now carrying out in different parts of the globe, supersede the necessity of a continuance of the observations made at the Apartments of the Royal Society, which could not be rendered so perfect as was desirable, on account of the imperfections of the locality and the multiplied duties of the observer.

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HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology or Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1847 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1844, and prior to the termination of the Session in June 1847.

The Council propose also to give one of the Royal Medals in the year 1847 for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1844, and prior to the termination of the Session in June 1847.

The Council propose to give one of the Royal Medals in the year 1848 for the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848.

The Council propose also to give one of the Royal Medals in the year 1848 for the most important unpublished paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848.

The Council propose to give one of the Royal Medals in the year 1849 for the most important paper in Physics, communicated to the Royal Society after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848, and printed in the Philosophical Transactions.

The Council propose also to give one of the Royal Medals in the year 1849 for the most important paper in Geology or Mineralogy, communicated to the Royal Society after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848, and printed in the Philosophical Transactions.

The Council propose to give one of the Royal Medals in the year 1850 for the most important paper in Mathematics, communicated to the Royal Society after the termination of the Session in June 1846, and prior to the termination of the Session in June 1849, and printed in the Philosophical Transactions.

The Council propose also to give one of the Royal Medals in the year 1850 for the most important paper in Chemistry, communicated to the Royal Society after the termination of the Session in June 1846, and prior to the termination of the Session in June 1849, and printed in the Philosophical Transactions.

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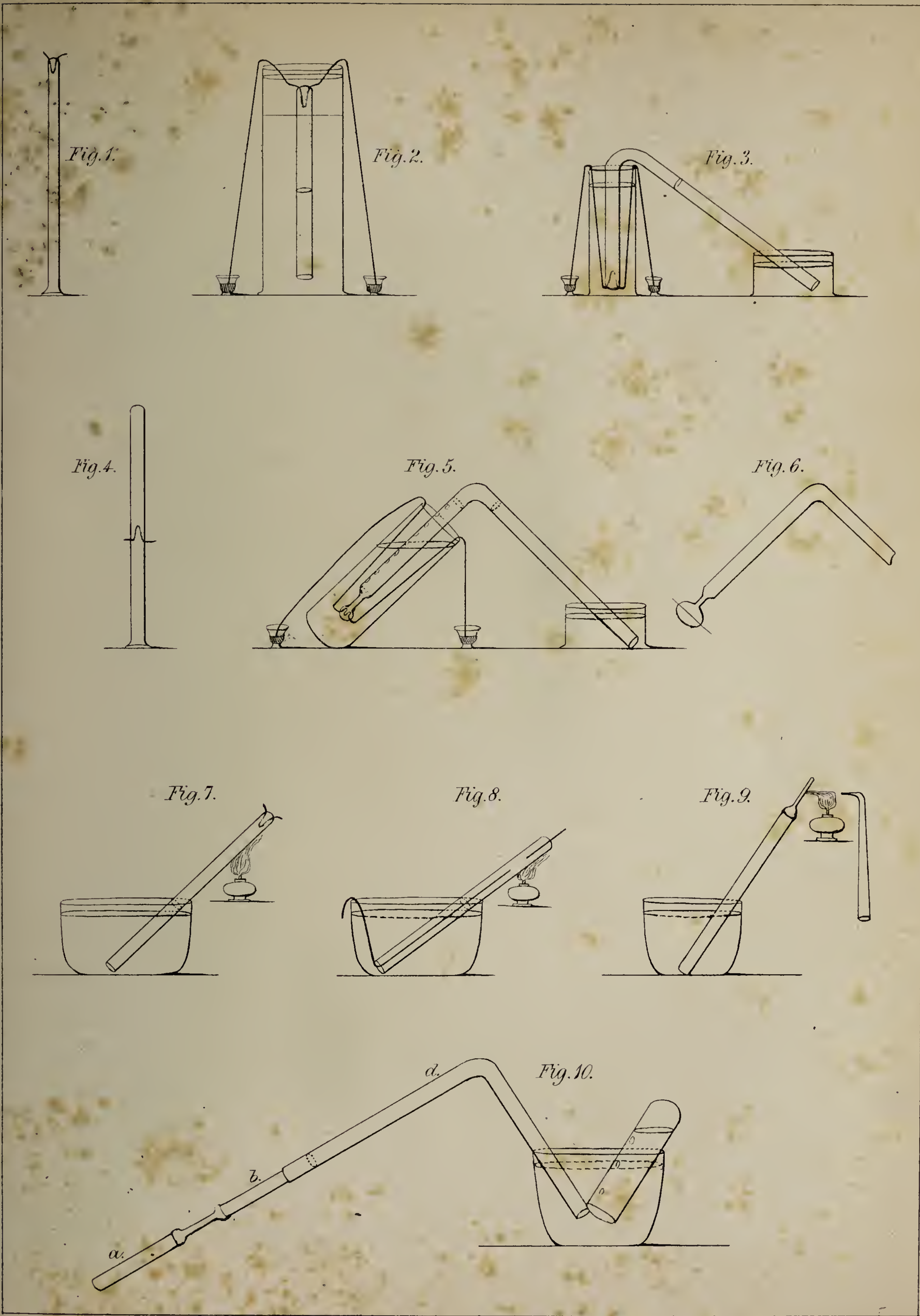

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PHILOSOPHICAL TRANSACTIONS.

I. The Bakerian Lecture.—*On certain Phenomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat.*

By W. R. Grove, *Esq., M.A., F.R.S.*

Received September 3,—Read November 19, 1846.

In the Philosophical Magazine for August 1841, I recommended for eudiometrical purposes, the use of a platinum wire ignited by a voltaic battery. In Plate I. fig. 1, is represented a form of apparatus for this purpose; it consists of a tube of Bohemian glass, with a loop of platinum wire $\frac{1}{80}$th of an inch diameter sealed into its upper end; the size of the glass tube may be adapted to the quantity of gas sought to be analysed, and may when necessary be reduced to extremely small dimensions, one-eighth of an inch being ample; into this the gas may readily be made to ascend, by the insertion of a wire of copper, platinum, or glass, as may be suitable to the gas: two cells of the nitric-acid battery are sufficient fully to ignite the wire, and the same battery supplies, by electrolysis, pure oxygen and hydrogen for the analysis. Since the period when I first proposed this, I have seldom used any other apparatus for such gaseous analyses as are performed by combining the gas to be examined with oxygen or hydrogen. This eudiometer possesses the advantage of enabling the operator either to detonate or slowly to combine the gases, by using different powers of battery, by interposing resisting wires, or by manipulation alone,—a practised hand being able by changing the intervals of contact to combine or detonate the gas at will. My general practice has been to produce a gentle heat in the wire until the gases contract, and then gradually to increase the heat until a full ignition takes place, by which means all the objects of the eudiometer of Volta are fulfilled, without detonation, without dependence on the fickle electric spark, and without thick tubes, any danger of explosion, or of the gases being projected from the eudiometer.

I have commenced with a description of this eudiometer, as it has been indirectly the means of my undertaking the experiments detailed in this lecture; and as its very great convenience has never been generally understood, I think that in strongly recommending it, I shall be of service to chemists.

In a paper honoured by insertion in the Philosophical Transactions for 1845, p. 358, I have shown another method of eudiometry also performed by voltaic ignition; in that experiment the vapour of camphor was decomposed into carbonic oxide and carburetted hydrogen; it was an application of voltaic ignition to effects analogous to those produced by PRIESTLEY and others, by passing compound gases through ignited tubes of porcelain.

But the voltaic process has this immense advantage, that the heat can be rendered incomparably more intense; that the quantity of vapour or gas to be operated on may be indefinitely small; that there are no joints, stop-cocks or ligatures; and that there is no chance of endosmose, which takes place through all porcelain vessels. I therefore determined to examine by these means several gases, both with a view of verifying, under different circumstances, known results, and seeking for new effects by this new and advantageous application. I used an eudiometer (fig. 1) of 8 inches long and 0·4 inch internal diameter, exposing the gases to intense heat, and subsequently analysed the residues in one of the same length, but 0·2 inch diameter.

I will first consider the physical effects of different gases on the ignition of the wire itself.

In a paper on the Application of Voltaic Ignition to lighting Mines*, I have mentioned the striking effects of hydrogen in reducing the intensity of ignition of a platinum wire, so much so that a wire voltaically ignited to incandescence in atmospheric air, is apparently extinguished by inverting over it a jar of hydrogen; with other gases the effects are not so striking, and with them these differences are best shown by including a voltameter in the circuit. DAVY found that the conducting power of a wire diminished in proportion to the degree to which it was heated: assuming the accuracy of this position, the amount of gas in the voltameter would be inverse to the intensity of ignition in the wire. The following is the result I obtained with different gases, employing the same battery (the nitric-acid combination at its most constant period), the same wire, and the same vessel:—

Gases surrounding the wire.	Cubic inches of gas evolved in the voltameter, per minute.
Hydrogen	7·7
Olefiant gas	7·0
Carbonic oxide	6·6
Carbonic acid	6·6
Oxygen	6·5
Compressed air, 2 atmospheres	6·5
Nitrogen	6·4
Atmospheric air	6·4
Rarefied air	6·3
Chlorine	6·1

* Philosophical Magazine, December 1845.

To ascertain the relation between the amount of radiant heat generated by the same battery and wire in gases which presented striking differences as to the luminous effects of the platinum wire, an apparatus was prepared in which the bulb of a thermometer was retained at a certain distance from the coil of wire ignited by a battery of four cells, and exposed, first, to an atmosphere of hydrogen, and then to one of atmospheric air, at the same temperature and pressure; the thermometer rose $7\frac{1}{2}°$ in five minutes in the hydrogen, and 15° in the air in the same time. Both the heating and luminous effects appear therefore to be greater in atmospheric air than in hydrogen. I cannot satisfactorily account for the differences shown in the above table; there appears a general tendency to greater ignition in the electro-negative than in the combustible gases, but the facts are far too few to found a generalization. I was at first inclined to regard the difference of effect in hydrogen as analogous to the peculiarity mentioned by LESLIE* respecting its convection of sound, but the parallel does not hold; sound is transmitted imperfectly through rarefied air, and also through hydrogen; on the contrary, the heat of the ignited wire is most intense in the former, and least so in the latter; the heat is also very much reduced in intensity in the compounds of hydrogen, ammonia and olefiant gas, or even by a small admixture of hydrogen with another gas, such as nitrogen; hydrogen, therefore, appears to have a peculiar and specific action in this respect.

I now pass to the consideration of the effects of the ignited wire on different gases. The ignition was in every case raised to the fullest extent, and the gases after exposure to it were carefully cooled down to their original temperature.

When the experiments were made over water, the whole eudiometer was immersed in a vessel of distilled water, occasionally having an inch depth of oil on the surface (see fig. 2†); when over mercury, and a long-continued exposure was required, a bent tube was employed, as at fig. 3, the closed end being immersed in water or oil, to prevent the fusion of the glass which would otherwise have ensued.

The tubes are much more easily preserved from cracking, and the ignition better kept up with oil on the exterior than with water, but as in many of these experiments I might have been considerably misled by a crack in the glass, or a bad sealing of the wire, allowing a portion of oil to enter the tube, I used water in the greater number of them until I was assured of the phenomena.

The apparatus, fig. 3, is superior in one respect to fig. 2, even for experiments over water, as the wire being situate below the volume of gas, the circulation is more rapid. This object may also be effected by employing the form of eudiometer, fig. 4, in which the loop of wire is near the centre of the tube, so as to be just above the surface of water in the tube; there are, however, some difficulties of manipulation with this form, which render it practically of less value than fig. 1.

* Transactions of the Cambridge Philosophical Society, vol. i. p. 267.

† In this and in figs. 3 and 5, the lines leading from the platinum loop to the mercury cups represent copper wires.

Binoxide of nitrogen over distilled water contracted differently in proportion to the heat of the wire; in the best experiment it contracted to one-third of its original volume; the residual gas was nitrogen. Nitric acid was found in solution in the water.

Over mercury the effects were nearly the same; the mercury was attacked, and the orange fumes of nitrous acid were visible.

Protoxide of nitrogen was decomposed into nitrogen and oxygen; the volume increased by 0·35 of the original volume; I could not get the full equivalent proportion, or 0·5 of oxygen.

Carbonic acid underwent no perceptible alteration.

Ammonia increased to double its original volume; it was now no longer absorbable by water, and gave 3 volumes of hydrogen, plus 1 nitrogen.

Olefiant gas contracted slightly, deposited carbon, the residue being hydrogen and olefiant gas, more of the former in proportion to the heat, but I could not succeed in entirely decomposing it.

Nitrogen suffered no change.

Oxygen gave a very slight contraction, amounting to $\frac{1}{50}$th of its volume; the oxygen employed was very pure, obtained from chlorate of potash and manganese, and also from water by electrolysis: no change in properties was perceptible in the oxygen after its exposure to the ignited wire. This contraction I incline to attribute to a slight portion of hydrogen present, which view will, I think, be considered as strengthened by the effect of the ignited wire on hydrogen, to be presently detailed. I at one time thought that the contraction might be due to a slight oxidation of the wire, but it never went beyond a very limited point; nor was the wire altered in size or weight, though it was kept ignited for many hours.

Chlorine over water gave dense white fumes; a greyish yellow powder accumulated on the sides of the tube near the platinum wire, which appeared of the same nature as the vapours; the deposit was insoluble in cold nitric, sulphuric, or muriatic acid, but dissolved by the last when boiled. The fumes did not, as far as I could judge, affect litmus paper; a barely perceptible tinge of red was indeed communicated to it, but this, I had every reason to believe, was attributable to a slight portion of muriatic acid not absorbed by the water. I have not yet worked out this result, as it is probable, considering the number of experiments that have been made on heated chlorine, that it is a known product, though I cannot find, in several books to which I have referred, any substance answering to it in description, and the field opened by voltaic ignition is so new that each result demands a separate and prolonged examination; if I find that this is an unknown compound I shall probably resume its investigation*.

Cyanogen gave, though in very minute quantities, a somewhat similar deposit, but at its then very high temperature it began to act rapidly on the mercury, and I was obliged to give up the experiment after an hour's ignition. Both these gases require peculiar

* See Supplemental Paper, p. 20.

and novel apparatus for examination by voltaic ignition. It will presently be seen that my whole attention and disposable time were necessarily occupied with certain phenomena to which this class of experiments ultimately led me.

Hydrogen gave a very notable contraction, amounting in some cases to one-tenth of its volume. This was an unexpected result, and I examined it with care. It took place both over water and over mercury; rather more with the former than with the latter. It obtained equally with hydrogen procured by electrolysis from carefully distilled water and pure sulphuric acid; with that procured from common zinc and pure sulphuric acid diluted with distilled water; and with that obtained from distilled zinc and pure diluted sulphuric acid. The contraction was less when the water from which the hydrogen was obtained was carefully purged of air by boiling and the air-pump, but yet there was a notable contraction even when the water had been freed from air to the utmost practicable extent. In the numerous experiments which I made on this subject, the contraction varied from the $\frac{1}{10}$th to the $\frac{1}{30}$th of the whole volume.

After many fruitless experiments I traced it to a small quantity of oxygen which I found hydrogen to contain under all circumstances in which I examined it. Phosphorus placed in hydrogen, obtained with the utmost care, gives fumes of phosphorous acid, shines in the dark and produces a slight contraction, but there is after this a further contraction by the use of the ignited wire.

I may cite the following as an easy experiment and simple illustration of the rapidity with which hydrogen appropriates oxygen. Let hydrogen be collected over water well-purged of air; let a piece of phosphorus remain in it until all combustion has ceased, the hydrogen will then be full of phosphoric vapour; fill another tube with water, and pass the hydrogen rapidly into it, the second tube will instantly be filled with a dense white fume of phosphorous acid; the hydrogen having instantly carried with it oxygen from the stratum of water it has passed.

A very careful experiment was made as follows:—distilled water was boiled for several hours, to this was added one-fortieth part by measure of pure sulphuric acid, and it was cooled under the receiver of an air-pump; it was now placed in two test glasses, connected by a narrow inverted tube, full of the same liquid: platinum electrodes were placed in each glass, and the hydrogen caused to ascend immediately into the eudiometer tubes; the whole was completed within two or three minutes after the water had been removed from the air-pump. Here the ordinary sources of impurity in hydrogen were avoided; no zinc was used, the sulphuric acid was pure, and the quantity was so small, that, had it not been pure, the error could have been but very trifling. The hydrogen so obtained, contracted in volume $\frac{1}{26}$th; hydrogen prepared in the same way, and exposed to phosphorus, gave dense white fumes; the phosphorus was luminous in the dark for more than an hour, and the contraction (temperature and pressure being carefully examined) was $\frac{1}{90}$th; the amount of contraction by the wire would of course equal three times the volume of oxygen mixed with the hydrogen, consequently the oxygen would be $\frac{1}{78}$th of the whole volume;

the platinum wire induces therefore a greater absorption of oxygen than the phosphorus, unless the volume of hydrogen is increased by the phosphoric vapour; the sequel of this paper will render it probable that even the ignited wire does not, and cannot induce combination of all the oxygen existing in the hydrogen.

I have looked into the papers of MM. Berzelius and Dulong, and of M. Dumas on the equivalent weight of hydrogen. The latter contains a most careful experimental investigation, and is by far the best determination we have; although it is not there mentioned that hydrogen contains oxygen, yet a correction is made for the air contained in the sulphuric acid employed. M. Dumas does not state how the quantity of that air is calculated. There can be no question that nothing approaching in elaborate care to these experiments has been yet performed on the subject, but with the fullest consciousness of M. Dumas' skill, I have, in all my experiments, perceived such an inveterate tendency of hydrogen to possess itself of oxygen, that I cannot help entertaining some doubts whether we have yet the real weight of hydrogen within the assigned limits of error.

It is difficult to see how hydrogen can be absolutely deprived of oxygen which has once existed in it; neither an oxidable metal as zinc, or an ignited inoxidable metal as platinum, getting rid of all the oxygen, and phosphorus, if it does so, replaces it by its own vapour. The near approach, however, of the equivalent of hydrogen, as determined by M. Dumas, to the ratio of whole numbers, renders it probable that it is a very close approximation to the truth.

I have not been able to detect nitrogen in the hydrogen, but the probability is that a slight quantity also exists in it. Whether the oxygen proceeds from portions of air still remaining in solution in the liquid from which the air is exhausted, or whether it is a part of the water actually decomposed, but of which the oxygen is not absorbed by the zinc, is a question to resolve which further experiments are necessary.

Hydrogen and carbonic acid mixed in equal volumes were readily acted on by the ignited wire; they contracted to 0·48 of the original volume; the residue was carbonic oxide; one equivalent of oxygen had therefore united with the hydrogen; and the slight additional contraction was probably due to the further combination of hydrogen with oxygen, as above stated.

Carbonic oxide exhibited a remarkable effect, and one which, coupled with the last experiment, gave rise to considerations which mainly led to the results to be detailed in the body of this paper. Carbonic oxide, very pure and carefully freed from carbonic acid, was exposed to the ignited wire over distilled water; the gas increased in volume in one experiment to one-third of its original volume, in the greater number of instances to one-fifth: this increase depended upon the intensity of ignition, which it was very difficult to maintain at its maximum on account of the frequent fusions of the platinum wires.

Here again I had a long research and many erroneous guesses, which I need not

detail. The effect did not take place with perfectly dry gas over mercury, and I thence was led to attribute it to some combination with aqueous vapour; the increase turned out to be occasioned by the formation of carbonic acid. By agitation with caustic potash or lime water the gas was reduced to exactly its original bulk, but it was now found to be mixed with a volume of hydrogen equal to the volume of carbonic acid by which it had been increased; it was thus perfectly clear that half a volume or one equivalent of oxygen derived from the vapour of the water, had combined with one volume or equivalent of carbonic oxide, and formed one volume or equivalent of carbonic acid, leaving in place of the carbonic oxide with which it had combined, the one volume or equivalent of hydrogen with which it had been originally associated.

Comparing the last experiment, viz. that of mixed carbonic acid and hydrogen with this, I was naturally struck with the curious reversal of affinities under circumstances so nearly similar; in the one case, hydrogen taking oxygen from carbonic acid to form water and leaving carbonic oxide; in the other, carbonic oxide taking oxygen from water to form carbonic acid and leaving hydrogen.

I thought much upon this experiment; it appeared to me ultimately that the ignited platinum had no specific effect in producing either composition or decomposition of water, but that it simply rendered the chemical equilibrium unstable, and that the gases then restored themselves to a stable equilibrium according to the circumstances in which they were placed with regard to surrounding affinities; that if the state of mixed oxygen and hydrogen gas were, at a certain temperature, more stable than that of water, ignited platinum would decompose water as it does ammonia.

This is a very crude expression of my ideas, but we have no language for such anticipatory notions, and I must adapt existing terms as well as I am able.

It now appeared to me that it was possible to effect the decomposition of water by ignited platinum; that, supposing the atmosphere of steam in the immediate vicinity of ignited platinum were decomposed, or the affinities of its constituents loosened, if there were any means of suddenly removing this atmosphere I might get the mixed gases; or secondly, if, as appeared by the last two experiments, quantity had any influence, that it might be possible so to divide the mixed gases by a quantity of a neutral ingredient as to obtain them by subsequent separation (or as it were filtration) from the neutral substance. Both these ideas were realized.

To effect the first object, after, as usual in such circumstances, much groping in the dark, I cemented a loop of platinum wire in the end of a tube retort similar to fig. 3, and covered it with asbestos, ramming this down so as to form a plug at the closed extremity of the tube, the platinum wire being in the centre. My object was, by igniting the platinum wire, to drain the water out of the asbestos, and the ignited wire being then in an atmosphere of steam, I hoped the water would by capillary attraction keep constantly oozing down to the platinum wire as the steam or decomposed water ascended. The experiment did not succeed; the water established a

current through the asbestos by washing away fine particles, and the phenomena of ordinary ebullition took place, unless the intensity of the battery was very much exalted, when a very slight decomposition was perceptible, which I attributed to electrolysis. This experiment, however, suggested another which did succeed. In one or two cases the asbestos plug became compressed above the platinum and so choked up the tube that the wire suddenly fused. It now occurred to me that by narrowing the glass tube above the platinum wire I had the result at my command, as the narrow neck might be made of any diameter and length, so as just to allow the water to drip or run down as the steam forced its way up; a tube was so formed, and is shown with its accompaniments at fig. 5.

The result of this experiment was very striking: when two cells of the nitric-acid battery were applied the air was first expanded and expelled, the water then soon boiled, and at a certain period the wire became ignited in the steam. At this instant a tremulous motion was perceptible, and separate bubbles of permanent gas of the size of pin-heads ascended, and formed a volume in the bend of the tube. It was not a continuous discharge of gas as in electrolysis, but appeared to be a series of rapid jerks; the water, returning through the narrow neck, formed a natural valve which cut off by an intermitting action portions of the atmosphere surrounding the wire; the experiment presented a novel and indescribably curious effect. The gas was oxyhydrogen. It will occur at the first to many of those who hear this paper read, that this effect might be derived from electrolysis. No one seeing it would think so for a moment; and although I shall by my subsequent experiments, I trust, abundantly negative this supposition, yet as this was my first successful experiment on this subject, and is *per se* an interesting and striking method of showing the phenomenon of decomposition by heat, I will mention a few points to prove that the phenomenon could not be occasioned by electrolysis.

To account for it by electrolysis, it must be supposed that the wire offered such a resistance to the current that this divided itself, and the excess of voltaic power passed by the small portion of water which trickled down, instead of by the wire.

In the first place, the experiment was performed with distilled water, and only two cells of the battery employed, which will not perceptibly decompose distilled water.

2ndly. No decomposition took place until the instant of ignition of the wire, though there was a greater surface of boiling water exposed to the wire before than after the period of ignition.

3rdly. A similar experiment was made, but with the wire divided in the centre so as to form two electrodes, and the water boiled by a spirit-lamp; here the current had no wire to conduct any part of it away, but the whole was obliged to pass across the liquid, and yet no decomposition took place, or if there were any it was microscopic.

4thly. When, instead of oil, distilled water was used in the outer vessel*, even the

* January 8.—I have since found that the exterior tube of oil or water may be dispensed with in this experiment, as the water which trickles down prevents the fusion of the glass.

copper wires, one of which would form an oxidable anode, gave no decomposition across the boiling water outside, while the ignited wire inside was freely yielding mixed gases.

5thly. To prevent the water from being the shortest line for the current, I repeated the experiment with a perfectly straight wire (fig. 6). The result was precisely the same; but the experiment is more difficult, as a certain length of wire is necessary, the sealing is more troublesome, and the size of the bulb is much more difficult to adapt to the production of steam in exactly the requisite quantity; the straight wire being more suddenly extinguished and more easily fused: with careful manipulation however it succeeds equally well with the former experiment.

I might add other experiments and arguments, but I believe when the remainder of this paper has been read, that the above will be thought scarcely necessary.

I now directed all my efforts to produce the effects by heat alone without the battery. I will mention a few of my unsuccessful attempts, as it will save trouble to future experimenters. I sealed a platinum wire into the extremity of a curved tube, filled the latter with water, and applied a strong heat by the blowpipe to the projecting end of the wire, hoping that the conducting power of the platinum, although inferior to that of most other metals, was sufficiently superior to that of glass to enable me to ignite the portion of the wire within the tube, and thus surround it with an atmosphere of steam; the water however all boiled off from the glass; nor could I succeed in igniting the platinum by heat from without. A similar failure occurred when, on account of its superior conducting power, a gold wire was substituted for that of platinum.

I sealed spongy platinum and bundles of platinum wire into the ends of Bohemian glass tubes, closing the glass over them, and then filling the tubes with water and heating the whole extremity; but the water boiled off from the glass, and the platinum could not be made to attain a full incandescence.

After many similar trials I returned to the battery, and sought to apply it in a manner in which electrolysis could not possibly take place. I had hoped, as I have above stated, to obtain a residual decomposition of water by masking or diluting the gases by a neutral substance. I therefore tried the following experiment: a tube similar to fig. 1 was filled with water which had been carefully freed from air by long boiling and the air-pump; it was then inverted in a vessel of the same water, and a spirit-lamp applied to its closed extremity, until the upper half was filled with vapour (see fig. 7). The wire was brought to a full ignition by the battery, and kept ignited for a few seconds; connexion was then broken and the lamp removed, so that the water gradually ascended. A bubble of the size of a large mustard-seed was left in the extremity of the tube, and I was much gratified at finding that when this was caught by a lighted match at the surface of the water-trough it detonated. The experiment was then repeated, continuing the ignition for a longer time, but the gas could not be increased beyond a very limited quantity; indeed it was not to have been expected, as supposing it to be mixed gas, recombination of the excess would have taken place,

and the fact of any uncombined gas existing when exposed to incandescent platinum, will doubtless surprise those who hear it for the first time.

The experiment was repeated as at first and the bubble transferred to another tube; the wire was then again ignited in vapour, another bubble was instantly formed and transferred, and so on, until after about ten hours' work sufficient gas was collected for analysis; this gas was now placed in an eudiometer, it detonated and contracted to 0·35 of its original volume; the residue being nitrogen. The experiment was repeated several times with the same general results, the residue sometimes containing a trace of oxygen.

Here electrolysis was out of the question; the wire was ignited in (if I may use the expression) dry steam, the upper part of the tube being far above the boiling-point, and of course perfectly transparent; if not an effect of heat, it must have been a new function of the electric current, at least one hitherto unknown.

As the voltaic arc and electric spark afford heat of the greatest intensity, I tried a succession of electric sparks from platinum wires through steam in the apparatus fig. 8, the water, as in all my experiments, having been previously purged of air (to save circumlocution I will in future call it prepared water). The sparks were taken from the hydro-electric machine of the London Institution; they had in the steam a beautiful crimson appearance; on cooling the tube a bubble was perceptible, which detonated by the match.

As in the previous experiments, a whole day's work did not increase the bubble, but when it was transferred another instantly formed; the gas was similarly collected; it detonated and contracted to 0·4 of its original volume; the residue was nitrogen with a trace of oxygen.

This experiment will again surprise by its novelty; the very means used in every laboratory to combine the mixed gases and form water, here decompose water*. From a vast number of experiments which I have made on the voltaic and electric disruptive discharges (which are I believe similar phenomena, differing only in quantity and intensity), I believe the decompositions produced by them are the effects of heat alone, and this experiment was therefore to my mind a repetition of the last under different circumstances; others however may think differently. This experiment also I several times repeated.

By counting the globules given off, and comparing a certain number of them with the average volume of steam in the last two experiments, an attempt was made to ascertain what proportion of water could be decomposed by an ignited platinum wire in aqueous vapour, or, which amounts to a corollary from this proposition, what degree of dilution would enable mixed gas to exist without combustion in an atmosphere of steam exposed to an ignited platinum wire. The proportion in an experiment in which the globules were so counted, was 1 to 2400; the probability is however that

* I need scarcely point out the distinction, in fact, between this experiment and those in which liquid water has been decomposed by the electric spark. See Supplemental Paper, p. 21.

different temperatures of the platinum wire would give different volumes of gas so decomposed, the volume being greater as the wire is more intensely ignited.

Although there was no known effect of electricity which could produce the phenomenon exhibited by the last two experiments, and it was in any event new, still, firmly convinced that it was an effect of heat, I again determined to attempt its production by heat alone, and without the use of the battery. I procured a tube of silver 9 inches long and 0·4 inch diameter; at the extremity of this was a platinum cap to which a smaller tube, also of platinum, was soldered. This platinum tube was closed at the end and soldered with gold solder. The apparatus was filled with prepared water; the water was boiled in the tube to expel the air from the narrow tube and any which might have adhered to the vessel; the tube was then, when full of hot water, inverted into water, and the flame of a common blowpipe made to play against the platinum tube (see fig. 9) until a white heat was obtained. Upon inverting it under water, a bubble of the size of a mustard-seed rose to the surface, which gave a very feeble detonation with the match. Similar bubbles were collected as before, and the gas in an eudiometer contracted to 0·7. On repetition the experiment did not succeed so well, and upon several repetitions it sometimes succeeded and sometimes failed, and I should not mention it but that it was the first experiment which gave me, although not very satisfactorily, the effect of decomposition by heat alone. The reason of its uncertainty I believe to have been the want of a sufficiently intense heat, as I dared not venture on account of the gold solder to push the ignition very far; in fact, I subsequently fused the extremity and spoiled the apparatus by applying the oxyhydrogen flame to it; had the platinum tube been welded instead of gold-soldered, it would doubtless have succeeded better. I should state that the object of the silver tube was to prevent the chance of recomposition by the catalytic effect of a large platinum surface; to have, in short, a small portion of platinum exposed to the steam, and that at a high temperature: economy was also no indifferent consideration. This experiment, although, coupled with the previous ones, tolerably conclusive, did not satisfy me, and I attacked the difficulty in another manner. The experiment (fig. 5) induced me to believe that if I could get platinum ignited under water so as to be in an atmosphere of steam, decomposition would take place; and M. Boutigny's experiments on the spheroidal state of water led me to hope I might keep platinum for some time under conditions suitable for my purpose.

After a few failures I succeeded perfectly by the following experiment. The extremity of a stout platinum wire was fused into a globule of the size of a peppercorn, by a nitric-acid battery of 30 cells; prepared water was kept simmering by a spirit-lamp, with a tube filled with water inverted in it; charcoal being the negative terminal, the voltaic arc was taken between that and the platinum globule until the latter was at the point of fusion; the circuit was now broken, and the highly incandescent platinum plunged into the prepared water: separate pearly bubbles of gas rose into the tube, presenting a somewhat similar effect to experiment (fig. 5).

The process was repeated, the globule being frequently plunged into the water in a state of actual fusion; and when a sufficient quantity of gas was collected it was examined, it detonated, leaving 0·4 residue; this was as usual nitrogen with a trace of oxygen. A second experiment gave a still better result, the gas contracting to 0·25 of its original volume.

On making the platinum negative and the charcoal positive, a very different result followed; the carbon was, as is known to electricians, projected upon the platinum; and the gas in this case was mixed with carburetted hydrogen and carbonic oxide. I know no experiment which shows so strikingly the different effects at the disruptive terminals as this; when the platinum is negative it gives much carbonic gas, when it is positive, not a trace (the gas was delicately and carefully tested for it); nay, more, by changing the platinum from negative to positive the carbon is instantly removed, and in a single experiment the platinum becomes perfectly clean.

Here then I produced very satisfactorily decomposition by heat; it is true, the battery was used, but used only as a means of fusing the platinum, as this was, as soon as fused, entirely separated from the circuit and could have no possible voltaic action. Wishing however altogether to avoid the use of the battery, I repeated this experiment, employing as my means of fusing the platinum the oxyhydrogen blowpipe; the experiment was equally successful, perhaps more so, as the manipulation was more easy.

I could readily by this means collect half a cubic inch or more of the gas; when detonated, the residue of nitrogen averaged 0·35 of the original volume.

In carefully watching this experiment, I observed that at first a rapid succession of bubbles ascended into the tube from the incandescent platinum, it then became quiescent; the spheroidal state was assumed by the water and no gas ascended; on losing the spheroidal state a sudden hiss was heard, and a single bubble ascended into the tube. I determined to examine separately the gas from the platinum before and after the quiescent state; to effect this I placed two inverted tubes in the capsule with the orifices near each other; the platinum at the point of fusion was immersed under one tube, say tube A, and as soon as the ascent of bubbles ceased, it was removed across to tube B, and the last bubble then entered that tube; the gases from each tube were separately analysed, and tube A gave nearly all detonating gas, the residue being only 0·2; tube B gave none; the gas collected in it was nitrogen, with a trace of oxygen.

In order to examine the effect of an oxidable metal under similar circumstances, I fused by the oxyhydrogen blowpipe the end of a stout iron wire, plunged it into prepared water and collected the globules of gas; no oxygen was given off, or at least no more than I have always found to accompany hydrogen, which with a small residue of nitrogen was the gas given off in this experiment.

I was now anxious to produce a continuous development of mixed gas from water subjected to heat alone, in other words, to succeed in an experiment which should

bear the same relation to experiment fig. 9 as fig. 5 did to fig. 7; for this purpose the apparatus shown at fig. 10 was constructed: *a* and *b* are two silver tubes 4 inches long by 0·3 inch diameter; they are joined by two platinum caps to a platinum tube *c*, formed of a wire one-eighth of an inch diameter drilled through its entire length, with a drill of the size of a large pin; *a* is closed at the extremity, and to the extremity of *b* is fitted, by means of a coiled strip of bladder, the bent glass tube *d*. The whole is filled with prepared water, and having expelled the air from *a* by heat, the extremity of the glass tube is placed in a capsule of simmering water. Heat is now applied by a spirit-lamp, first to *b* and then to *a*, until the whole boils; as soon as ebullition takes place, the flame of an oxyhydrogen blowpipe is made to play upon the middle part of the platinum tube *c*, and when this has reached a high point of ignition, which should be as nearly the fusing-point of platinum as is practicable, gas is given off, which, mixed with steam, very soon fills the whole apparatus and bubbles up from the open extremity, either into the open air or into a gas collector. Although by the time I had devised this apparatus I was from my previous experiments tolerably well assured of its success, yet I experienced a feeling of great gratification when on applying a match to one of the bubbles which were ascending, it gave a sharp detonation; I collected and analysed some of it; it was 0·7 oxyhydrogen gas, the residue nitrogen, with a trace of oxygen.

Those who have endeavoured to deprive water of air, will have no difficulty in accounting for the residual nitrogen, or nitrogen mixed with a small portion of oxygen, which has occurred in all my experiments. De Luc pointed out the impossibility of practically depriving water of air, and Priestley, from observing the obstinacy with which water retained air, was led to believe that water was convertible into nitrogen (phlogisticated air). I have repeated several of Priestley's experiments under much more stringent circumstances, and have never been able to free water from air, or so to boil water that for every ebullition of vapour a minute bubble of permanent gas was not left, which appeared to have been an indispensable nucleus to the vapour.

The difficulty of boiling water increases, as M. Donny has proved, in proportion to its freedom from air, and at last the bursts of vapour become so enormous that the vessels employed are generally broken. There appears to me a point beyond which this resistance does not extend; but even at this point a minute bubble of air is left for each burst of vapour, though they are so few and distant that the aggregate amount of gas is very trifling. I have produced from water which had been previously carefully deprived of air by the ordinary methods, three-fourths of its own volume of permanent gas, which proved to be nitrogen; but as the water in this experiment was boiled under a long column of oil, it is probable that if any oxygen were present, it might have been absorbed by the oil; I have, however, always found the proportion of oxygen to decrease as the boiling was continued. It may be worth noticing, as having had some influence on my mind, that many months ago, when considering the experiments of Henry and Donny on the cohesion of water, I men-

tioned to Mr. Gassiot, and also to Mr. Bingham my assistant (to whose assiduity I am much indebted), that I was inclined to think if water could be absolutely deprived of air, it would be decomposed by heat, a result which I have now attained by a totally different series of inductions. It is a circumstance worthy of remark, that I find the greater part of the air to be expelled at a comparatively low temperature, and when the water has come in contact with the platinum, while the decomposition all takes place when the platinum is surrounded by an atmosphere of steam, if steam it may be called, for the state of this atmosphere at the first immersion of the platinum is at present very mysterious.

I think I may now safely regard it as proved, that platinum intensely ignited will decompose water, and several considerations press on the mind in reflecting on this novel phenomenon.

First of all, to those who are attached to the *cui bono* argument, and estimate physical science in proportion only to its practical applications, I would say that these experiments afford some promise of our being, at no distant period, able to produce mixed gases for purposes of illumination, &c. by simply boiling water and passing it through highly ignited platinum tubes, or by other methods which may be devised; we in fact by this means, as it were, boil water into gas, and there appears theoretically no more simple way of producing chemical decomposition.

To pass however to more important considerations: the spheroidal state, which has lately attracted the attention of philosophers, appears to be closely connected with these results, and is rendered more deeply interesting. The last experiment but two which I have mentioned, shows that the spheroidal state is intermediate between ordinary ebullition and the decomposing ebullition; it is probably therefore a state of polar tension, coordinate in some respects with that which takes place in the cell of a voltaic combination before decomposition, or when the power employed not being of sufficient intensity to produce actual decomposition, the state commonly called polarization of the electrodes, obtains. The phenomenon brings out also a new relation between heat, electricity, and chemical affinity; hitherto many electrical phenomena could be produced by heat and chemical action, the difference being that in the effects produced by the last two forces there was no polar chain, but every minute portion of the matter acted on gave rise to the phenomena which in the electrical effects are only observable at the polar extremities; thus in decomposing water by iron and sulphuric acid, or by passing steam over heated tubes of iron, parallel results are obtained to the electrolysis of water with an iron anode; but in the former cases every portion of the iron oxidated gives off its equivalent of hydrogen, in the latter the equivalent is evolved from the cathode at a point distant from that where the oxidation takes place. Hitherto electricity has been the only force by which many compounds, and particularly water, could be resolved into their constituents without either of these being absorbed by another affinity. The decomposition by ignited platinum removes this exception, and presents the parallel effect produced by heat alone.

Although there is no substance except platinum and some of the more rare metals, such as iridium, which promise much success in a laboratory experiment made for the purpose of producing the effect I have described, as the greater number of substances which will bear a sufficient heat, are fragile, oxidable, or affected by water, yet general considerations from the nearest analogies in chemistry would lead us to expect a similar effect from all matter in a state of intense ignition; even assuming the presence of solid matter to be necessary, the catalytic effects of platinum are shared in different degrees by other substances: it therefore appears probable that at a certain degree of heat water does not exist as water or steam, but is resolved into its constituent elements. If, therefore, there be planets whose physical condition is consistent with an intense heat, the probability is, that their atmosphere and the substances which compose them are in a totally different chemical state from ours, and resolved into what we call elements, but which by intense heat may be again resolved into more subtle elements. The same may be the case in the interior of our planet, subject however to the counter agency of pressure.

The experiments strongly tend to support the views of Berthollet, that chemical and physical attraction are affinal, or produced by the same mode of force. All calorific expansions appear to consist in a mechanical severance of the molecules of matter; and if heat produce effects of decomposition merely by increase of intensity, there seems no reason why we should assign to it in this case a different mode of action from its normal one. On this view physical division carried on indefinitely must ultimately produce decomposition, and chemical affinity is only another mode of molecular attraction. Thus a high degree of rarefaction, as at the bounds of the atmosphere, or in the interplanetary spaces, may entirely change the chemical condition of matter.

In a paper published in the Philosophical Transactions for 1843, p. 111, I have shown that we may oppose a chemical action by a physical one (electrolysis by a vacuum), that antagonizing chemical by physical tension, they mutually oppose each other. I believe the converse of this experiment has been made by M. Babinet, who by physical compression has prevented the development of chemical action.

I have also described in the Philosophical Magazine for November 1845, certain phenomena which appear to me to be irreconcileable with received chemical views; and though I then believed that the theory of Grotthus would be obliged to give way, I now incline to think that some of our chemical doctrines must ere long undergo a revision.

It is rather surprising that the valuable applications of which the phenomena of voltaic ignition are capable, and the fertile field which (as I believe) it presents for discoveries, both physical and chemical, should have been so completely neglected. It is true that until a recent period the imperfection of the voltaic battery rendered accurate and continued experiment on this subject difficult of performance, but still much might have been done. Davy made several experiments on the voltaic disrup-

tive discharge, which in many points may be regarded simply as very intense ignition; but I am only aware of two experiments of his on voltaic ignition; one, in which he employed it in an exhausted receiver to examine to what extent the radiation of heat was carried on *in vacuo*; and another, already alluded to, in which, by immersing a portion of an ignited wire in water, he observed that it conducted in some inverse ratio to its heat.

I have made a vast number of experiments on the voltaic arc or disruptive discharge, in various media*; when this is taken in a medium incapable of acting chemically on the electrodes, the phenomena are those of intense ignition of the terminals, which are dissipated in vapour and condensed upon the interior of the vessel in which the discharge is taken. I have examined some of these deposits, and they appear to consist of the metal of the terminals in a finely-divided state; this is strikingly shown with zinc. If the arc be taken between zinc points in an exhausted receiver, a fine dark powder, nearly black, is deposited on the interior, which, when collected, proves to be pure zinc, and on the application of a gentle heat, takes fire in the open air and burns into the white oxide: to casual observation the zinc would appear to be burned twice. The experiment appears to me to present an argument in favour of the dynamic theory of heat.

With charcoal, on the other hand, there is little or no deposit, but the charcoal continually yields carbonic oxide and hydrogen, and this for hours after the presence of water would be deemed impossible. I have taken the arc between pieces of well-burned charcoal for eight or nine successive hours, and there was still gas generated; indeed it appeared to be given off as long as there was any charcoal remaining, and a conversion of the carbon into inflammable gas might have been supposed. Much still remains to be done with this powerful agent, the voltaic arc: where, however, the object is simply to expose gases to an intense heat, the ignition of a conjunctive wire of platinum is more simple in its application, more uniform in its action, and instead of requiring a powerful battery, the effect can be satisfactorily produced by five or six cells, in many cases by two.

The heat is not so intense as that of the arc, but as it can be brought to within a few degrees of the fusing-point of platinum, it is far more intense than any heat usually employed in laboratories, certainly than any which can be applied to minute, I may say microscopic portions of gas or vapour.

In conclusion, I must express my sincere thanks to the managers of the London Institution, for having permitted me, as an honorary member, to carry on these experiments in the laboratory of the Institution.

* Philosophical Magazine, June 1840. Literary Gazette and Athenæum, February 7, 1845.

London Institution,
August 21, 1846.

II. *Supplementary Paper on certain Phenomena of Voltaic Ignition, and the Decomposition of Water into its Constituent Gases by Heat.* *By* W. R. Grove, *Esq.*

Received November 26,—Read November 26, 1846.

In selecting the above title, I endeavoured to give as clear an enunciation of the phenomena to be described in the paper as was consistent with the brevity usual in a title.

An exception has, however, been taken to it, that as the effects of decomposition are produced by ignited platinum, the phenomena may result from that obscure mode of action called catalysis. That I did not intend to exclude from consideration any possible action of the substance employed, will be evident from the paper itself, in which I have called attention to the general production of catalytic effects by solid bodies.

Whatever value or novelty there may be in the facts I have communicated, is the same whether they be regarded as resulting from catalytic or from thermic actions. If the action be catalytic, it is one absolutely the reverse of that usually produced by platinum, and therefore just as much at variance with received experience as decomposition of water by heat would be; the effect of platinum, like that of heat, on the elements of water having been hitherto known only as combining them. With regard to any theoretic views I may have advanced, I by no means attach the same importance to them as I do to the facts themselves, though I consider it necessary for the collation of facts, and desirable for the progress of science, that an author pretending to communicate new results should give with them the impressions which led to their discovery, and the inferences which he regards as immediately deducible from them. No expression can be given to facts which does not involve some theory, and admitting the difficulty (perhaps insuperable) of correctly enunciating new phenomena, and the probability of future discoveries entirely changing our views regarding them, I cannot at present see that the title of my paper could be altered without being open to greater objections. I am of this opinion, not so much because other bodies than platinum will produce the effect, as I shall presently show, nor from the fact that the electrical spark will decompose aqueous vapour, though these are arguments in its favour; but from the following considerations. The catalytic action of platinum will induce or enable combination to take place where there is already a strong affinity or tendency to combine, as with mixed oxygen and hydrogen gases; it will also induce decomposition where the affinities are extremely weak, or in a state of unstable equilibrium, as in Thenard's peroxide of hydrogen; again, where there are

nicely-balanced compound affinities, it may change the chemical arrangement of the constituents of a compound, but I do not know of any case in which a powerful chemical affinity can be overcome by catalytic action; to effect this we require some natural force of greater intensity than that to be overcome. We might as well say that the platinum electrodes of a voltaic battery decompose water, as to say that platinum decomposes it in the case in question; there, the force of electricity acts only by means of matter, and matter of a peculiar description; its action also is only perceptible at the surface of this matter. I seek to use the expression in my title with reference to heat in a similar sense to that in which we use similar terms with reference to electricity, *i. e.* to regard heat as the immediate dynamic force which overcomes the affinity; thus, as we say when employing the voltaic battery, that we decompose water by electricity, so here we should say that we decompose it by heat.

If it be said that heat so weakens or antagonizes the affinity of the elements of water as to enable catalytic action to separate them, this amounts to the same theory, as heat is then regarded as the antagonizing force, and in this case the action, both thermic and catalytic, is the reverse of the normal action. I have thought it desirable shortly to discuss this question as likely to lead to further investigation, though I have been somewhat embarrassed by the want of definite meaning in the term catalysis; I must plead guilty to have frequently used the term, but notwithstanding, or perhaps on account of, its convenience, it has I fear had an injurious effect on scientific perspicuity.

The following experiments were made to ascertain whether platinum was the only substance by which the effect could be produced. A knob or button of the native alloy of iridium and osmium of the size of a small pea was formed by the voltaic battery; to this was attached by fusion another smaller knob of the same metal one-fourth the size of the former, and to this smaller one was attached a stout platinum wire; the object of the second knob was both to prevent the fusion of the platinum wire and also to avoid the possibility of any surface of platinum being exposed to the recipient tube or alloyed with the metal to be heated. The preparation of this simple instrument was very troublesome, but when made it answered the purpose well; the larger button could be fully ignited to an intense glow, while on account of the narrow neck which united them, the smaller was barely red-hot, and the platinum wire not perceptibly ignited. An experiment having been made with this metallic button and prepared water, similar to that previously made with platinum, gas was given off which averaged 0·3 of mixed gas; the residue was nitrogen mixed with varying small quantities of oxygen. The effect, upon the whole, was decidedly inferior to that of the platinum. Indeed as platinum is the most dense and unalterable of all known substances, it would be likely, upon any received theory of heat, to produce the greatest effects.

I tried palladium in the same manner; the gas yielded was hydrogen with small quantities of oxygen, and the water was stained with the oxide o the metal.

I now tried silica and other oxides, but the results were not very satisfactory. A spheroid of silica was formed by fusing pulverized silica on to a platinum wire, so as to cover it for the length of 0·4 of an inch; when this was plunged into the hot water and again fused in the oxyhydrogen blowpipe, it constantly became frothed with small bubbles of vapour, and after a few experiments generally separated in fissures; in the experiment which was continued for the longest time without disintegration, the gas given off contained 0·15 of oxyhydrogen gas; from the whole result I believe there is an action of the water on the silica (probably forming a hydrate decomposable by heat) which is a bar to satisfactory results. With other oxides, at least such as would bear an intense heat, the difficulties were still more insuperable. PRIESTLEY has shown that water will corrode glass, and if I mistake not, others have shown the same effect produced on silica.

Although, as applied to the facts detailed, I attached no further meaning to the title of my paper than that which I have above stated, yet in one or two theoretical inferences I have certainly gone further; for instance, when I suppose the possibility or probability of mechanical rarefaction producing the same effects as heat, here (although I do not, indeed I cannot conceive the existence of heat without matter) I certainly abstract from the proposition any consideration of solid matter. In order to ascertain how far this view might be founded on truth, I had thought of making a few experiments on the effect of mechanical rarefaction on the tendency of gases to combine, but (in addition to the interference of necessary occupations) I find that M. DE GROTTHUS has already experimented on the point; his experiments, as far as they go, corroborate the views I have put forth.

He finds* that mixed gases, such as chlorine and hydrogen, or oxygen and hydrogen, when rarefied either by slow increments of heat or by the air-pump, do not take fire (" ne s'enflamment pas") by the electric spark. From the context, he evidently means that the gases will not detonate or unite in volumes, as he states that a partial combination ensues. GROTTHUS appears to have considered the combination of gases by the electric spark as an effect of sudden compression or molecular approximation, certain particles being brought within the range of their affinities by the sudden dilatation of others. Although he did not pursue the subject far enough to ascertain whether a degree of rarefaction could be reached which would be an actual bar to combination, still his experiments strengthen those views which assimilate mechanical and thermic molecular repulsion, and regard chemical affinity as being antagonized by physical repulsion.

Pursuing the series of analogies from the decomposition of euchlorine at a low temperature, that of ammonia at a higher, that of metallic oxides at a higher, and so on to oxide of hydrogen, there appears to be an extensive series of facts which afford strong hope of a generalized antagonism between thermic repulsion and chemical

* Annales de Chimie, vol. lxxxii.

affinity, and a consequent establishment of the law of continuity in reference to physical and chemical attraction.

The deposit from chlorine, to which I have alluded in my paper, I have since examined, and though it differs in colour from that described in books, I find it is a protochloride of platinum, formed at the expense of the platinum wire. The larger portion of the chlorine in the tube combines with the hydrogen of the aqueous vapour, and the muriatic acid is absorbed by the water; when the experiment terminates the gaseous volume is reduced to nearly one-half, and this residue is oxygen.

This effect induced me to try an ignited wire on other analogues of chlorine, and I tried bromine and chloride of iodine in the apparatus (fig. 5). The tube was filled with the liquid, and its extremity was in the first experiments immersed in another narrow tube of the same liquid as that which filled it. When the platinum wire was ignited, permanent gas was given off both from the bromine and from the chloride of iodine, which gas on examination proved, to my surprise, to be oxygen. In one experiment I collected half a cubic inch of gas from an equal volume of chloride of iodine. As the experiment in this form required too large a quantity of the liquid to enable me to observe any change which might take place in its character, I repeated it with a tube five feet long, bent in two angular curves. A small quantity of the liquid was placed in the extremity of the tube containing the wire, which was so arranged as to be the lowest point; the angles were placed in cold water and the experiment proceeded with; my object was to enable the dense vapour of the liquids to shelter them from the atmosphere, there being no satisfactory method of shutting them in and yet allowing room for the elimination of the liberated gas, or of absorbing the latter by combination without also absorbing the vapours.

I had hoped by the above means to proceed with the experiments until all the oxygen was liberated that could be driven off, and then to have examined the residua; but I found that after experimenting for a short time, both the platinum wire and the glass in proximity to it were attacked by the liquids; this difficulty, similar to those which have hitherto prevented the isolation of fluorine, I have not yet been able to conquer, though I hope to resume the experiments.

As chloride of iodine is decomposed by water, it cannot contain any notable quantity of the latter, but, until the experiments are carried further, it must remain a question whether the oxygen results from a small quantity of water contained in the liquid, the hydrogen combining with the liquid itself, or from a decomposition similar to that of the peroxides. The experiments certainly add a new and striking analogy to those already known to exist between the peroxides and the halogens, but they do not, as far as I have hitherto carried them, necessarily prove analogy of composition.

In conclusion, I would call attention to a point which I omitted to notice in my original paper, viz. the explanation afforded by the results contained in it of the

hitherto mysterious phenomena of the non-polar decomposition of water by electrical discharges, as in the experiments of PEARSON and WOLLASTON. This class of decompositions may now be carried much further. With the exception of fused metals, I know of no liquid, which, when exposed to intense heat such as that given by the electric spark, the voltaic arc, or incandescent platinum, does not give off permanent gas; phosphorus, sulphur, acids, hydrocarbons, water, salts, bromine and chloride of iodine, all yield gaseous matter.

Viewing these effects simply as facts, and without entering on any theoretical explanations or speculations, I cannot but think that there is a remarkable generality pertaining to them worthy of the most careful attention.

The apparatus I have described, particularly that represented by fig. 5, and the numerous applications of voltaic ignition which will occur to those who duly consider the subject, promise, I venture to believe, new methods and powers of investigating the molecular constitution of matter, and will, I trust, lead to novel and important results.

Nov. 10, 1846.

III. *Microscopic Observations on the so-called Vesicular Vapours of Water, as existing in the Vapours of Steam, and in Clouds, &c.* *By* A. Waller, *Esq.*, *M.D.* *Communicated by* P. M. Roget, *M.D.*, *Sec. R.S.*

Received April 13,—Read June 18, 1846.

In a paper published in the Philosophical Magazine, February 1846, respecting some molecular actions of crystalline particles, I endeavoured to explain the fixation of particles of mercury, and the consequent formation of images in the Daguerreotype process. My experiments led me at that time to the conclusion, that the vapours, or rather fumes of condensed steam, consist of minute globules or spherules of water, and not of small vesicles according to the theory universally received at present, and expressed by the term of *vesicular vapour*.

My opinion on this point was founded on the results obtained by microscopic inspection of the condensed vapours of mercury and of other bodies, at the same time I stated that I was unable in the case of water to confirm it by direct observation. Subsequently, however, I have succeeded in doing so, and propose to show from the following observations that the opinions entertained respecting vesicular particles of water are completely erroneous. But before I describe my own experiments, I think it proper to state some of the ideas of our predecessors in science on vapours of water, and likewise the grounds on which they supported them.

Sir Isaac Newton, on various occasions in his Optics, mentions the particles composing clouds, fogs and mists, &c., so as to leave no doubt that he considered them to be composed of minute globules or spherules, as may be seen in the following passages:—"Between the parts of opaque and coloured bodies, are many spaces either empty or replenished with mediums of other densities; as water between the tinging corpuscles wherewith any liquor is impregnated, air between the *aqueous globules that constitute clouds and mists*."—Prop. III. Book 2nd. "But when in order to compose drops of rain they begin to coalesce and constitute globules of all intermediate sizes, these *globules*, when they become of a convenient size to reflect some colours and transmit others, may constitute clouds of various colours according to their sizes. And I see not what can be rationally conceived in so transparent a substance as water for the production of these colours, besides the various sizes of its fluid and *globular* particles."—Prop. V. In noticing the coloured areola surrounding the sun and moon in certain circumstances, he does not fail, when giving the measurement of their diameters, to mention the way in which they may be applied in obtaining a knowledge of the relative diameters of the globules of water which give rise to them.

M. Kaemtz has obtained several interesting results in making use of this mode of observation, which has so long been neglected by meteorologists; by means of these rings he has ascertained that the average sizes of these particles vary in the different months of the year, and that in summer they are much smaller than in winter; the mean size he assigns to them is about $0{\cdot}0224^{mm}$.

The opinion now entertained respecting these particles of water, consists in regarding them as composed of small bladders or vesicles, similar in all respects, but their size, to the common soap-bubble. This theory is generally ascribed to Halley. Neither Newton nor Halley appears to have taken any steps to submit their hypotheses to direct observation. This was first attempted by Kratzenstein, whose experiments on this subject, dating from 1743, evince great ingenuity. The original work is very scarce in France, and not to be obtained in this country. But De Saussure, in his 'Traité sur l'Hygrométrie,' has cited them textually, from whence I extract the following. "M. Kratzenstein qui s'est beaucoup occupé de ces vésicules et qui a même prétendu réduire à elle seules tous les genres de vapeurs, a tenté de les mésurer et les a comparés avec un cheveu, et il a même cru pouvoir assurer que leur diamètre était douze fois plus petit. Le cheveu avait suivant M. K. $\frac{1}{300}$ de pouce et par conséquent ces vésicules une 3600 de la même mesure*."

Kratzenstein, after measuring the diameter of these supposed vesicular globules, attempted to determine the thickness of the pellicle which he believed to exist around them, and thus states his experiment in § 4 of his dissertation:—"J'ai pris un globe de verre qui avait cinq pouces de diamètre; à son orifice était adapté un robinet. En soufflant dans le globe j'ai comprimé l'air qui y était contenu, puis ayant fermé le robinet j'ai exposé le globe aux rayons du soleil dans la chambre obscure; mais je n'ai pu appercevoir aucune des vapeurs que j'avais fait entrer en y soufflant. Ayant ouvert le robinet pour faire sortir l'air comprimé, j'ai vu d'abord une grande quantité de vapeurs qui tombaient; mais elles ont encore disparu lorsque j'ai comprimé de nouveau l'air qui était dans le globe. Regardant de manière que mon œil fit avec le rayon du soleil un angle entre 5 et 10 degrés, j'ai apperçu avec un grand plaisir une suite de très belles couleurs qui se changaient peu à peu en d'autres à mesure que l'air comprimé sortait de la boule. Voici la suite de couleurs telle que je l'ai remarquée, rouge, verd, bleuâtre, rouge, verd. Ayant mis mon œil entre le soleil et les vapeurs et les ayant regardé sous les mêmes angles que je viens de dire, j'ai apperçu les mêmes couleurs que donnaient la réflexion, mais elles étaient dans un ordre inverse." Kratzenstein supposes that these colours arose from the expansion of the vesicles which caused their parietes to become thinner, and to assume colours in the same way as the soap-bubble; he calculated their thicknesses accordingly, which on an average he found to be about $0{\cdot}06^{mm}$. De Saussure, in quoting this experiment, states that it never succeeded in his hands, for the colours were simul-

* Théorie de l'élevation des vapeurs et des exhalaisons démontré mathématiquement, par M. Gottlieb Kratzenstein. Bordeaux, 1743.

taneous and not successive, as is required. These appearances are easily explained in the present state of science, which could not be done in the time of KRATZENSTEIN. The condensation of the vapours of water is caused by the sudden demand for caloric by the rarefied air in the glass globe, and the same takes place in the receiver of the pneumatic machine when there is a commencement of a vacuum. As the gas within recovers its caloric from surrounding bodies, the globular vapours and the moisture deposited on the inner surface of the glass recover their elastic condition. With regard to the colours observed, they are owing to the *diffraction of light* by the particles in suspension, and more particularly by those parts condensed on the sides of the glass, as in some of FRAUNHOFER's experiments. Therefore it is in no manner possible to connect these colours either with the thickness of the vesicles, or with the vesicular theory.

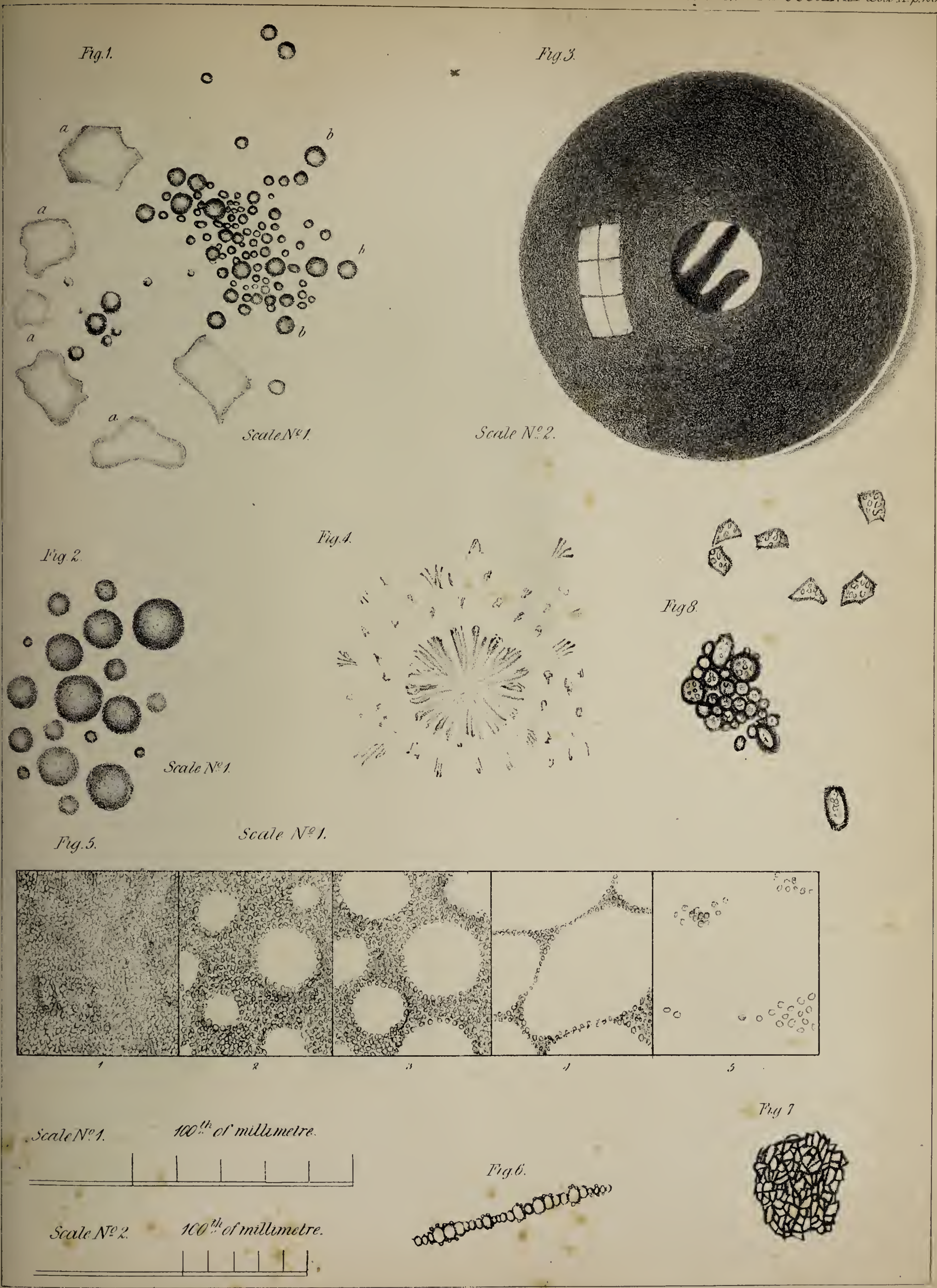
I have related these experiments thus fully, because I find that even at the present day, they still pass current with observers of a deservedly high reputation. Thus they are reproduced in KAEMTZ's Treatise on Meteorology, perhaps the most profound work on the science, and now rendered popular in this country by Mr. WALKER's translation of his Manual.

DE SAUSSURE, on examining with a lens of an inch focus the steam of water floating over a dark surface, in speaking of the vesicles, says, "La légéreté de ces petites sphères, leur blancheur, leur apparence absolument différente de celles des globules solides, leur parfaite ressemblance avec les bulles volumineuses que l'on voit nager à la surface du liquide ne laissent aucun doute sur leur nature; il suffit de les voir pour être convaincu que ce sont des sphères creuses, semblable à la grosseur près à celles que l'on forme avec l'eau de savon." He further states that he confirmed these ideas by the examination of fogs or clouds on high mountains, when he was enabled to perceive in the same way minute vesicles, sometimes accompanied with drops or globules of water. He estimates the smallest at $\frac{1}{4500}$th of an inch, and the largest at $\frac{1}{2780}$th. When these vesicles came in contact, they burst and formed a small drop of water. These experiments have been adopted and reproduced by most modern authors, among whom may be mentioned BERZELIUS, FRESNEL, MITSCHERLICH, &c.

As I have already stated in the Philosophical Magazine, I had an opportunity of repeating DE SAUSSURE's observations on the clouds, at the Monastery of St. Bernard. I will quote the passage: "Globules of various sizes are frequently discerned by the naked eye floating in all directions. I have endeavoured to ascertain their vesicular structure, but have been unable to do so from direct observations. It is frequently a most difficult point in microscopic investigation to decide upon the existence of a thin transparent membrane. It is still more so to pronounce upon the vesicular or spherular structure of globules in constant agitation; and I believe that if minute spherules and vesicles could be mixed together, we do not possess any means at present of distinguishing them. I have never been able to detect that appearance

of bursting of the globules mentioned by DE SAUSSURE, but sometimes when the agitation of the air is slight, two of the larger globules may be seen floating towards each other, and afterwards disappear suddenly, which may be explained, if we admit that it is caused by the union of the two spherules into one, which is too heavy to remain any longer in suspension, and whose rapid deposition conceals it from the sight." From the commencement of my experiments, I found that the greatest obstacle to a perfect investigation of these globules arose from their excessive mobility. Even to the naked eye this is very great, and is the cause of several optical delusions; but by the use of magnifying instruments, this mobility must necessarily be increased in proportion to their power. With the simple microscope of DE SAUSSURE, a globule might occasionally be perceived, and its diameter estimated to a certain extent; but with the compound achromatic microscope I employ, their angular displacement is too rapid to form any correct sensation on the retina. For this reason I was led to adopt the plan of fixing the condensed vapours arising from the breath or other sources, in some liquid which, like oil, possesses no affinity for water. The liquids with which I thus mixed the vapours of water were very numerous; and as the appearances thus presented under the microscope were not the same in all cases, I will only mention some of the most interesting.

Canada balsam is, perhaps, of all other vehicles, the most adapted for these observations. A slip of glass covered with a thin layer of it is to be used. By breathing on it with a little force, the vapours of the breath will not only be condensed on its surface, but will penetrate below, where they may be easily recognized in opake streaks of a white colour. By reason of the viscousness of the balsam, they will remain almost stationary for more than an hour, and for a much longer period if covered with another thin piece of glass or talc. Under the microscope these streaks are decomposed into minute globules perfectly spherular like shot, or the globules of mercury. See Plate II. fig. 1.*bb*. Their sizes vary within certain limits, between which they occur, of all intermediate-sizes. They agglomerate together in various ways, forming lines composed of several rows of them touching each other, but without any signs of coalescing, unless attentively examined for some time, when two smaller ones may be perceived to disappear, and in their place is seen another of larger size resulting from their fusion. A certain degree of attraction exists between them, as they are generally packed together as closely as possible, and any hiatus caused by any of them coalescing is quickly filled by others. Thus a group will sometimes have its form modified and reduced by their pressing inwards to supply any hiatus left after one or two have coalesced. Another proof of their mutual attraction is offered in the movement *en masse* of a single file of globules without their separating from each other. In favourable circumstances, I have found some of them still in "statu quo" after a lapse of twenty-four hours or more. Their diameters vary generally from $0{\cdot}001^{mm}$ to $0{\cdot}003^{mm}$. They disappear at length by adhering to and wetting the surface of the glass above or below them, where they may easily escape notice, as from



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their flattened surfaces they possess but little power to deviate the light from its regular course. Their shape is very irregular, generally of an angular, and frequently of an hexagonal form. See fig. 1. *aa*. That part condensed on the surface of the balsam presents exceedingly minute globules of the same nature.

Instead of condensing the breath as above, I have placed the slip of glass covered with balsam in the open air, on grass covered with hoar-frost, and after it had become slightly turbid on its surface from the moisture deposited, I submitted it to the microscope. It was then seen to be covered with lines of single globules like strung beads, following the most tortuous courses; and wherever the balsam had been disturbed by the passage of any body through it even for a long time previous to the exposure, the direction of these lines showed the manner in which it had been disturbed. The diameters of some of these globules appeared as mere points under the strongest magnifying power, and were probably less than $0{\cdot}0001^{mm}$. As the glass became warm the globules coalesced at intermediate points into a single one, which soon vanished in its turn. When steam from boiling water in a glass tube was condensed in Canada balsam, the globules which are represented in fig. 2 are much more voluminous, varying from $0{\cdot}01^{mm}$ to $0{\cdot}015^{mm}$, though in other respects they were exactly the same. The globules arising from water at 60° Reaumur are the same size as those of the breath.

No doubt can possibly be entertained, after having inspected these globules attentively, of their being perfect spherules of liquid without any central part whatever, for containing a gasiform fluid, which could not escape detection when they coalesce and adhere to the glass. Globules of gas, of whatever nature, cannot possibly be confounded with one of these liquid globules, from their being much darker, and the smallest of them considerably larger than those of water, independently of their remaining permanent when inclosed between two surfaces of glass. In Canada balsam they present, on a very minute scale, the exact representation of external objects which surround the microscope, which, however, is seen on liquid globules when they are sufficiently large. Thus, in looking through the microscope, I have seen at the lower part of one of these air-globules portions of my own face or body; and in the same way an extensive view of trees, houses, &c. may be traced with all its details on one of these microscopic globules not larger than $0{\cdot}01^{mm}$ or $0{\cdot}02^{mm}$. Fig. 3 represents the image of part of the hand, as seen in a globule of air, drawn under the camera lucida. I mention this fact, as I believe that the telescopic action of the microscope, by means of these hollow prisms, may be capable of several interesting applications.

By breathing gently on the surface of Canada balsam, it will become covered with a film of moisture, reflecting various colours in proportion to the quantity deposited. Any bright object viewed through it will appear surrounded by a halo of brilliant colours, such as those seen sometimes encircling the sun or moon. These coloured films, under the microscope, are decomposed into colourless particles of water, which

are all of the same form, although an irregular one, not completely circular. If a surface of balsam be placed aside for a short time and afterwards examined, we find that these particles, which before were disposed in a regular manner, appear to have altered their position, and that they have grouped themselves in radiating lines towards a variety of points. See fig. 4.

Particles of water may be examined in a solid and crystalline state. A slip of glass or a small glass tray, coated with Canada balsam on its under surface, and a freezing mixture at the upper, will gradually condense the moisture from the air, and the balsam will become covered with the well-known spongy deposit of frozen particles. The manner in which the crystallization takes place is remarkable. At first the moisture condensed is liquid and globular in form, but as the refrigeration continues a sudden molecular change occurs, and the globules are found to assume various crystalline shapes. When they are small and numerous, on consolidating, they form a kind of areolar net-work, the appearance of which is caused by the reflexion of numerous crystalline facets (see fig. 7); at other places may be seen small pyramidal crystals covering a solid globular nucleus like the head of a mace; sometimes they assume the form of small aciculæ, of hexagonal prisms with various secondary facets, of octohedrons, &c. At the moment when this molecular action takes place, I have sometimes perceived with the microscope a kind of flitting movement, as the globules are assuming their various shapes, like that which occurs in the crystallization of particles from a state of solution*.

The globules which compose mists or fogs may be condensed by exposing a slip of glass coated with balsam in the open air, but as the deposition takes place very slowly, it is preferable to employ the rotatory bellows, so placed as to throw the current of air at an angle of about 60° on the plate. Steam or other volatile substances may be condensed in the same manner. When the current is created by the common bellows, I have found it much more difficult to fix the globules. Those obtained by this process are exactly similar to those of steam. In my experiments they were between $0{\cdot}02^{mm}$ and $0{\cdot}03^{mm}$ in size.

Essence of turpentine may be used in the same manner as Canada balsam. The globules condensed in this manifest much greater mobility, as might be expected from the nature of the liquid, frequently rebounding from each other as they come in contact. They disappear very rapidly, either by adhering to the glass, or by coalescing together. I have observed them moving in vortices around several central points where they rapidly collapsed. Their diameters vary between the

* The most favourable season for performing these experiments is the winter. In summer, when the air is warm and loaded with moisture, the particles on condensing, form liquid or solid globules, or very confused crystals, and whenever the freezing mixture is moved aside, so as to expose a portion of the under surface to microscopic inspection, a very few moments suffice for the particles to liquefy. Solid globules may be distinguished from liquid ones by their borders being much darker, and from their frequently containing smaller globules, as in fig. 8.

$0{\cdot}001^{mm}$ and the $0{\cdot}003^{mm}$. Essence of turpentine is still better adapted than the balsam for retaining the globules suspended in the air, as in fogs, &c. When, after having been cooled below the common temperature, it is breathed upon, a whitish precipitate of a streaky appearance is formed like an insoluble salt, which slowly descends to the bottom of the vessel, where it coalesces into larger globules*. This is also the case with water-globules condensed in this way from the air, which form a cloudy precipitate intercepting the passage of light. On being preserved in a bottle this cloud will still remain after the lapse of a couple of days, when the water will be found at the bottom of the vessel in a globular shape like metallic mercury, without adhering to the sides of the glass. By means of this permanent cloud, we might be enabled to perform a series of experiments of great interest to meteorology. In this way we might determine the influence of water-globules of various sizes in the passage of luminous, thermic, and chemical rays.

Copaiva balsam, upon being breathed upon, becomes coated with a milky film, which, to the naked eye, assumes a reticulated appearance after a few moments, which is caused by the disappearance of the film sooner at some points than at others. Under the microscope the uniform film is decomposed into minute globules, which occupy the space of mere points under the strongest magnifying power. They disappear very rapidly by agglomerating and forming larger globules. This process of agglomeration commences simultaneously on the surface of the liquid at a variety of different points. From these it proceeds to form circular areas, which continue to extend until their line of demarcation consists of merely a single row of globules, which also in their turn contract and coalesce into small groups of about a dozen separate spherules. At this time the process of fusion ceases, and the globules are seen suddenly to fly apart in virtue of some molecular repulsion, and then gradually disappear. With boiling water the like phenomena are produced, except that the globules are all much larger. In fig. 5 are seen these actions in all their different stages.

Oil of peppermint condenses moisture which presents phenomena very similar to those of copaiva balsam. If the breath be passed through a narrow tube, a system of rays is produced, composed of separate globules, which remind one of the form of some of the species of Asterias which radiate from a centre, and are likewise composed of a multitude of separate globular pieces. The irisated colours formed on this oil are very brilliant, but soon disappear unless the current of air is constantly kept up. The particles of water which give rise to these colours, disappear by immersion into the oil, as well as with all the other oils which are susceptible of presenting them. The oils of anethum, carraway and cajeput, also present very similar appearances.

* A bottle containing condensed water-globules and essence of turpentine, exposed night and day in a northern aspect, became coated at its inner surface on the north side with condensed moisture, and around this with transparent acicular crystals arranged in zones around the moisture. The crystals were not obtained in sufficient quantity to admit of their being analysed.

Creosote condenses the breath, which agglomerates into globules apparent to the naked eye and long remains unaltered.

Olive oil.—The globules are very minute and agglomerate slowly. The surface assumes a lacerated appearance, and the molecular dispersion appears simultaneously over the entire field of vision. Croton oil condenses a film reflecting the various spectral colours.

Castor oil.—The condensed particles of water present the same appearance to the naked eye, or when assisted by the microscope, as the two preceding. If, before the globules have disappeared, they are covered over with another slip of glass having a film of oil previously placed on it, they are retained for a much longer period than if the surface of the glass had been perfectly dry. Almost all of them are adherent together, as in fig. 6, but without agglomerating into one. In some instances a larger globule, like a central nucleus, is seen covered with others adhering to it. Their appearance reminds one strongly of the manner in which the condensed vapours of sulphur are found adhering together.

Another means of subjecting particles of water to microscopic observation, consists in fixing them upon minute filaments, such as those of the spider's web. I have found it most convenient to employ the filaments which contain the ova of the spider, or those of the cocoon of the silk-worm. Thin bundles of either of these exposed to steam from boiling water, were found after a few moments to have very minute globules condensed upon them, mostly imperceptible to the naked eye, about the same size as those obtained in Canada balsam from boiling water. These particles of water were all liquid globules without any signs of vesicular structure. In foggy weather the filaments of the spider's web generally become covered with small globules of water, which in some cases are so minute as to give the filament a grey whitish aspect, like globules floating in the air, or in spirit of turpentine. These filaments, fixed between two small frames pressed close together, may be examined under the microscope. They are then seen covered with globules of water, and with others of an organic nature secreted by the spider. The former may readily be distinguished, by their quick evaporation, from the others which are permanent. The globules of water were about $0{\cdot}02^{mm}$.

We may therefore conclude from the foregoing observations, that the term vesicular structure of globules, which was first proposed at a time when the knowledge of gaseous bodies was still in its infancy, has been adopted without sufficient foundation; that the experiments on which it was grounded were unsatisfactory and imperfect; and that whenever we are enabled to inspect the minutest particles of water arising from condensed steam or vapour, they consist of minute liquid globules without any appearance of internal cavity.

Kensington, April 12*th*, 1846.

IV. *Quelques recherches sur l'Arc Voltaique, et sur l'influence qu'exerce le Magnétisme soit sur cet arc soit sur les corps qui transmettent les Courants Electriques Discontinus.*

Researches on the Voltaic Arc, and on the influence which Magnetism exerts both on this Arc and on bodies transmitting interrupted Electric Currents. By M. Auguste De la Rive, *Professor in the Academy of Geneva, Foreign Member of the Royal Society, Corresponding Member of the Academy of Sciences at Paris, &c. &c.*

Received November 26, 1846,—Read January 7, 1847.

THE luminous voltaic arc occurring between two conducting bodies, each communicating with one of the poles of the pile, is not merely one of the most brilliant phenomena in physics, but, from the numerous aspects under which it may be regarded, it is also one of the most important.

As a source of light, this phenomenon, when exhibited in a vacuum, enables us to examine what influence this particular origin of the light employed may have in various optical experiments. Compared with the solar light, the light of the voltaic arc presents some curious differences and also resemblances. If, on the one hand, we find in it the seven coloured rays of the spectrum, on the other the black streaks are replaced by brilliant ones, and these are differently interspaced. In this field of inquiry, much, or rather all, yet remains to be investigated.

As a source of heat, the voltaic arc enables us to study the fusion and solidification of even the most refractory bodies *in vacuo*, and consequently under circumstances exempting them from oxidizing action and other chemical influences, which usually result from the application of a high temperature in atmospheric air. It likewise allows us to determine the effects produced upon bodies at a high temperature, by various gases or vapours, distinct from those which enter into the composition of atmospheric air, and at different degrees of density.

As an electro-chemical power, the voltaic arc may be applied so as to submit to the electrolizing action of the electric current gaseous media, which, from some experiments already made, appear capable of decomposition by this process.

As a mechanical power, the voltaic arc, by bringing bodies into a state of minute division, and impressing upon them, in this state, a tendency to motion, places them in a favourable condition for the study of their molecular constitution, and of the relations which connect this constitution with electricity and magnetism. The struggle that takes place between cohesion and the expansive force of the electric

current, the reduction of matter to the molecular state, and the form and nature of the deposits resulting therefrom, are so many phenomena capable of throwing light on the obscure subject of molecular physics.

The few preceding remarks suffice to give some idea of the extent of an investigation embracing the whole range of experimental research on the voltaic arc under its various aspects, which I am far from pretending to have attempted. I shall confine myself at present to a few details, and especially to such as exhibit the action of magnetism on the voltaic arc, and on those bodies which transmit interrupted currents. I shall begin by describing some particular phenomena which I observed during my study of the voltaic arc under various circumstances, while employing different substances as electrodes, both in the air and in a vacuum ; I shall then proceed to examine the action of a powerful electro-magnet on this voltaic arc, and I shall conclude by describing some remarkable experiments also illustrating the influence of magnetism on conducting bodies, of whatever nature, traversed by interrupted currents.

§ 1. *Some Phenomena concerning the Voltaic Arc.*

Davy was the first who produced the phenomenon of the voltaic arc with two points of charcoal. More recently, Messrs. Grove * and Daniell † employed with success the points of different metals, and arrived at interesting results : I also published some experiments I made on the voltaic arc ‡ in 1841. Subsequently, MM. Fizeau and Foucault observed some remarkable facts of the same kind on the occasion of an investigation into the intensity of the light emitted by charcoal in the experiment of Davy §. The researches made up to the present time, have already led to many results, of which I shall consider only the most important.

1. That the voltaic arc may be produced, a pile of greater tension is required than that which is necessary for the ordinary calorific and electro-chemical phenomena. The necessity of this condition proves the great resistance presented to the passage of the electric current by the minutely divided matter, whatever it may be, which connects the two poles.

2. The luminous arc cannot exist, unless contact be previously made between the electrodes, and unless these, or at least one of them, be terminated at the point of contact by points fine enough to produce in them an increase of temperature. When this increased temperature is once produced, we may, by separating the electrodes gradually and with precaution from each other, obtain the luminous arc, the length of which will depend on the intensity of the pile. Daniell discovered the important fact, which was confirmed by M. Van Breda in a very recent investigation inserted in the Comptes Rendus de l'Académie, that without contact having taken place, the luminous arc may be produced between two electrodes placed very near together, by causing the discharge of a Leyden jar to pass between them : this is owing to the

* Bibl. Univ. June 1840, i. 27. p. 387.

† Arch. de l'Elect. tom. i. p. 462.

‡ Arch. de l'Elect. tom. i. p. 262.

§ Ibid. tom. iv. p. 311.

discharge being always attended by the transference of highly diffused matter, which closes the circuit during the instant of time necessary for the formation of the arc.

3. The enormous elevation of temperature which accompanies the production of the luminous arc, is also manifested in the electrodes, especially in the positive ones, which become much more strongly heated than the negative.

4. Matter is thus transported from the positive electrode to the negative, a fact which may be verified with electrodes of all kinds, but particularly with those of charcoal.

5. The various phenomena presented by the voltaic arc, are modified to a greater or less extent by the nature of the electrodes and by that of the surrounding medium. Thus Mr. Grove adduces facts from which it appears that the presence of oxygen is necessary in most cases to produce a very luminous and brilliant arc. It results also from his experiments, as well as those of other philosophers, that when two different substances are made use of for the electrodes, it is not a matter of indifference which of the two is placed at the positive pole.

I now proceed to my own researches. I commenced by studying the production of a luminous arc between a plate and a point of the same material in air, and *in vacuo*. By means of a micrometer screw I was able to make the point recede from the plate very gradually, and judge of their mutual distance with great precision. The limit of distance beyond which the luminous arc ceases to appear, is constant for the same plate and the same point: when, however, the plate communicates with the positive pole, it is in general double that which it is when the point communicates with the same pole. But in proportion as the strength of the pile is greater, the difference is so much the smaller.

With respect to the absolute amount of this distance, it is very variable, depending on the strength of the pile, on the nature and molecular state of the electrodes, and on the time occupied in the experiment. Thus, with a Grove battery composed of fifty pairs of plates sixteen square inches in surface, it is two or three times greater than with a pile of seventy elements of two or three square inches. With metals easily fused or oxidized, as zinc and iron, it is much greater than with platinum or silver. The duration of the phenomenon influences the result, inasmuch as the high temperature of the electrodes allows them to be drawn asunder to a greater distance without breaking the arc. The same effect may be produced by heating them artificially, by means of a spirit-lamp. It is evident from what I have said that the length of the luminous arc has a relation to the greater or less facility which the substances composing the electrodes possess of being segregated, a facility which may depend upon their temperature diminishing their cohesion, upon their tendency to oxidize (which produces the same effect), upon their molecular state, and lastly upon their peculiar nature. Carbon derives from its molecular constitution, which renders it so friable, the property of being one of the substances which produces the longest luminous arc.

The deposits of the transported matter, form upon the plate, when it is negative and the point positive, a species of very regular ring, the centre of which is the projection of the point upon the plate. This takes place equally, whether the plate be vertical or horizontal, plainly indicating a determinate direction in the transfer of the substance from the positive to the negative electrode; in the air and with metallic electrodes, the deposits always consist of the oxidized dust of the metal, of which the positive electrode is composed.

I shall here enter into some details. A plate and a point of platinum have been used as electrodes in a vacuum, in air and in hydrogen. In a vacuum with a Grove battery of fifty pairs of plates, which had previously been used, I had only a very feeble effect, and particularly when the plate served as the positive electrode. The point was hardly removed a millimetre* from the plate when the arc broke; to re-establish it, it became necessary to renew the contact between the point and the plate, by touching another point of the plate, the first point which was touched appearing to have undergone such a modification as to prevent the re-formation of the arc. The same effect is produced when the experiments are made in the air, but it ceases when the power of the battery is increased: this is probably due to an augmentation of cohesion consequent on the increase of temperature in that part of the plate which acts as the positive electrode. Besides, when the experiment is made in air, the voltaic arc is more marked and of greater length than when it is made *in vacuo*, at least if the battery be weak; for when the battery is powerful, composed, for example, of fifty pairs of plates freshly charged, it appeared to me that the contrary obtained. I did not, however, perceive any great difference; but the vacuum in which I experimented was far from being perfect; it was that of a pneumatic pump, enclosing therefore highly rarefied air.

In the latter case, that is to say, with the pile composed of fifty pairs strongly charged, and in highly rarefied air, a bluish spot, perfectly circular and presenting the appearance of a coloured ring of Nobili, was formed on the plate of platinum when it served as the positive electrode. The same spot appeared in atmospheric air, but its diameter was one-half less, and its colours much less vivid. In hydrogen, no coloured spot was formed; its formation is therefore evidently the result of the oxidation of the platinum at a high temperature when acting as a positive electrode in the ordinary atmosphere, and still more so, perhaps, in rarefied air†. When the same plate of platinum was made use of as a negative electrode, the point being positive, it became covered with a white circular spot, formed of a vast number

* 1 millimetre = 0·03937 inch.—Trans.

† This effect may possibly have been owing to the action of the oxygen brought by the voltaic current into that particular state which Schönbein first described under the name of *ozone*. Indeed, in this state the oxygen may attack those metals which are supposed to be inoxidizable; and M. Marignac and I have shown that this may be effected by causing a succession of electric discharges to pass through the oxygen, even when very dry, with which the phenomenon of the voltaic arc has a great resemblance.

of minute grains of platinum, which, having been raised to a high temperature, remained adhering to the surface. The white spot, like the blue one, was much larger in rarefied air than in a vacuum. If the experiment be prolonged for a minute or two when the plate is negative, the rod of platinum terminating in a point, which is positive, soon becomes highly incandescent; its end is fused and falls on the plate in the form of a perfectly spherical globule. When the plate is positive and the point negative, the latter is less heated, and does not become fused; but the plate, unless it be very thick, is liable to be perforated: besides, as may easily be imagined, the phenomenon lasts much longer in the latter case. The light is less brilliant, but it is accompanied by a reflexion of a superb blue, which may be seen when the experiment is made in the interior of a bell, whether the air be rarefied or not. This blue reflexion is observed on the side of the bell, and is to be seen whatever may be the nature of the electrodes, or the colour of the light to which these give rise in the centre of the bell; only when this central light is very brilliant, it becomes slightly paler by the effect of contrast.

I substituted for the platinum point a point of coke, but the plate of platinum remained; this being positive and the point negative, I obtained a luminous arc more than double the length of the arc produced by the point of platinum. With respect to the arc, instead of its being a cone of light, having its base on the plate and its apex at the point, as was the case when the latter was platinum, it was composed of a multitude of luminous jets diverging from different points of the plate, and tending to various parts of the point of coke. This fact shows clearly the influence that may be exercised by the negative electrode, the function of which is very far from being a merely passive one. Let me add, that although the strength of the pile was precisely the same as when the point was of platinum, not only was the luminous arc much longer with the point of coke, but the heat developed in the plate of platinum was so much greater that it was soon melted and perforated. The coke being positive and the plate negative, the length of the arc was less than in the preceding case, and particularly so in air, where it was sensibly less than in a vacuum. The heat generated was however still very great, the point of coke becoming quickly incandescent throughout. I ought to add, that with the point of coke, the luminous arc was so brilliant that the blue light which I have mentioned almost entirely disappeared, which was not the case with any other kind of point.

Leaving the plate of platinum, I adjusted a zinc point. The effects were most brilliant, but of short duration, the point speedily melting. In common air, a deposit of white oxide was precipitated upon the platinum plate; in highly rarefied air (the vacuum of an air-pump), a black deposit was formed: in both cases it communicated with the positive pole. An iron point being substituted for that of zinc, equally produced in common air a brownish red deposit of oxide of iron, and in rarefied air a deposit of black oxide.

I call the attention of chemists to these two facts, as well as that of the oxidation

of the platinum at a high temperature in rarefied air. They appear to prove the influence which the state of greater or less density of the surrounding oxygen may exert on the phenomenon of oxidation and on the nature of the oxide formed. A plate and a point of soft iron were used as positive and negative electrodes, both in a vacuum and in the atmosphere; the same results appeared with a plate and a point of silver, a plate and a point of copper, and a plate and point of argentane*. The blue light was perceived in all the experiments; coloured circles were likewise seen on all the plates when they had acted as positive electrodes in rarefied air. The silver and copper plates presented in this case very decided cavities, caused by the passage of the matter from the positive to the negative pole. The points became incandescent throughout when they served as positive electrodes; whereas when negative, they were heated only at their extremities. The copper point when positive became isolating at its extremity, and it was necessary to excite it by friction in order to renew the experiment. This circumstance is probably attributable to the formation of a thin film of oxide. The point and plate of copper gave out a luminous arc of a beautiful green light, which contrasted in a remarkable manner with the blue reflexion visible in this, as in the other experiments. Mercury was likewise employed, both as a positive and negative electrode. In a vacuum as well as in atmospheric air, the luminous effect was most brilliant. The mercury was excessively agitated, rising up in the form of a cone when it was positive, and sinking considerably below the positive point when it was negative. The quantity of vapour thrown off by the mercury during this experiment filled the bell so quickly that it was not easy to observe the details.

I shall terminate this section by stating a fact which appears to me to be important; it is the influence which the nature of the metallic points forming the electrodes exercises on the temperature which they acquire in relation to the production of the voltaic arc. If the two points are of the same metal, both platinum, or both silver, the positive one alone becomes incandescent throughout its whole length. If the silver point be positive and that of the platinum negative, the latter becomes incandescent, and the silver one is much less heated. Thus, when the voltaic arc is formed, the circuit must be regarded as completed, and then it is those parts of the circuit which present the greatest resistance to the current which become the hottest; at first it is that portion forming the arc itself, and then, in the rest of the circuit, the metal which is the worst conductor. But if the conductors be of the same material on both sides of the arc, or if there be only a slight difference of conductibility between them, then the development of heat, instead of being uniform, as it might appear it ought to be, is much greater on the positive side. This important fact evidently proves that this portion of the circuit has to resist a much more energetic action than that which the other side experiences; a fact which is confirmed by the molecular segregation accompanying this action at the positive electrode. This want

* An alloy of copper and nickel: also known by the names of *pachfong* and *melchior*.

of resemblance in the phenomena presented by the two electrodes, although placed in conditions entirely symmetrical, deserves to be taken into serious consideration, for it may throw light upon the nature of the electric current, and upon the link which unites it with the molecular state of the bodies through which it is transmitted.

§ 2. *Influence of Magnetism on the Voltaic Arc.*

Davy was the first who observed that a powerful magnet acts upon the voltaic arc as upon a moveable conductor, traversed by an electric current; it attracts and repels it, and this repulsion and attraction manifests itself by a change in the form of the arc. Even the action of the magnet may, as I have found, break the arc by too great an attraction or repulsion exerted upon it, causing the communication which the transmitted particles establish between the electrodes to cease.

The action which I have just mentioned is not the only one which magnetism exerts on the voltaic arc. I have already stated the curious fact, that if two points of soft iron acting as electrodes, be both placed within a helix formed of thick copper wire of several coils, the voltaic arc developed between the two points of iron ceases the moment a strong current is passed through the wire of the helices, and reappears if this current be arrested before the points have become cold. The arc cannot be formed between the two iron points when they are magnetized, whether by the action of the helices, or by that of a powerful magnet, unless they be brought much nearer to one another, and the appearance of the phenomenon is then entirely different. The transported particles appear to disengage themselves with difficulty from the positive electrode, sparks fly with noise in all directions, while in the former case, it was a vivid light without sparks, and without noise, accompanied by the transfer of a liquid mass, and this appeared to be effected with the greatest ease. It is of little moment with respect to the result of the experiment, whether the two rods of magnetized iron present to that part of their extremities between which the luminous arc springs, the same magnetic poles or different poles.

The positive electrode of iron, when it is strongly magnetized, produces, the moment that the voltaic arc is formed between it and a negative electrode of whatever nature, a very intense noise, analogous to the sharp hissing sound of steam issuing from a locomotive engine. This noise ceases simultaneously with the magnetization.

For the purpose of better analysing these different phenomena, I placed an electro-magnet of large dimensions and great power in such a manner as to enable me to place on each of its poles, or between them, different metals destined to form one of the electrodes of the pile, while one point of the same metal, or another substance, acted as the other electrode. I have alike employed as electrodes, placing them in the same circumstances, two points of the same metal or of different metals. The following are the results which I have obtained. A plate of platinum was placed on one of the poles of the electro-magnet, and a point of the same metal was placed vertically above it; the voltaic arc was produced between the plate and the point, the

plate being positive and the point negative. As soon as the electro-magnet was charged, a sharp hissing was heard; it became necessary to bring the point of the plate nearer to enable the arc to continue, and the bluish circular spot which the platinum plate presented, became larger than when the experiment was made beyond the influence of the electro-magnet. The plate was made negative, and the point positive; the effect was then totally different; the luminous arc no longer maintained its vertical direction when the electro-magnet was charged, but took an oblique direction, as if it had been projected outwards towards the margin of the plate; it was broken incessantly, each time accompanied by a sharp and sudden noise, similar to the discharge of a Leyden jar. The direction in which the luminous arc is projected, depends upon the direction of the current producing it, as likewise on the position of the plate on one or other of the two poles, or between the poles of the electro-magnet. A plate and a point of silver, a plate and a point of copper, and generally a plate and a point of any other metal, provided it be not metal too easily fused, present the same phenomena.

Copper, and still more silver, present a remarkable peculiarity. Plates of these two metals retain on their surfaces the impression of the action that took place in the experiments just described. Thus, when the plate is positive, that portion of its surface lying beneath the negative point presents a spot in the form of a helix; as if the metal melted in this locality had undergone a gyratory motion around a centre, at the same time that it was uplifted in the shape of a cone towards the point. Moreover, the curve of the helix is fringed throughout by minute ramifications, precisely similar to the tufts which mark the passage of positive electricity in a Leyden jar. When the plate is negative and the point positive, the marks are totally different, being merely a simple point, or rather a circle of a very small diameter, whence proceeds a line more or less curved, forming a kind of tail to the comet, of which the small circle might be the nucleus: the direction of this tail depends upon the direction in which the luminous arc has been projected.

When, instead of a plate and a point, two points are used for electrodes, it is evident that no visible trace of this phenomenon can be obtained; but both the sharp hissing and the detonations may be produced, which latter are sometimes so loud as to bear a resemblance to distant discharges of musketry. For this the electro-magnet must be very powerful, and the current which produces the arc very intense. I had observed that when I took for a positive electrode a point of platinum, and for a negative electrode a point of copper, and placed them between the two poles of the electro-magnet, the production of the voltaic arc between the two poles was accompanied by a sharp hissing noise; whereas in the opposite case, the copper being positive, and the platinum negative, the detonations were heard, attended by a frequent breaking of the arc. On examining this phenomenon more closely, I perceived that the fact I have just mentioned was due to the platinum becoming heated much more rapidly than the copper when they were employed as electrodes in producing the voltaic arc; and

I have satisfied myself that in order to obtain the hissing sounds, it is necessary that the positive electrode should be at a sufficiently high temperature to experience a commencement of liquefaction; for without this condition, only a series of detonations are heard. The hissing would be the result of the easy and continuous transport of matter more or less liquefied from the positive electrode, whilst the detonations would be the effect of the resistance opposed by the same matter to the disintegration of its particles when it is not sufficiently heated. Numerous experiments made with metallic points, whether of the same or different natures, as silver, iron, brass, as also platinum and copper, some of which become heated sooner than others under the same circumstances, have quite confirmed me in this view of the subject. It is merely necessary to be careful, in order to produce the hissing noise, to maintain as much as possible the continuity of the arc when once the positive electrode becomes incandescent; while, on the other hand, to obtain the detonations, one of the electrodes must be held in the hand, and then the arc frequently made and broken without waiting till the metallic points acquire too high a temperature.

It remains now to be considered why the influence of powerful magnetism, such as that exerted by the electro-magnet, is necessary for the production of these sounds, which are not heard in the ordinary experiment of the voltaic arc. This can arise only from the change which the magnet produces in the molecular constitution of the matter of the electrode, or rather in the highly diffused matter which forms the voltaic arc. This action is besides shown by the shortening of the arc, and by the remarkable difference which it presents in its appearance; it is therefore not surprising that it should also be capable of producing a phenomenon such as sound, which essentially depends on the variations in the molecular state of bodies. This view of the subject appears to me to deserve very particular attention: the results at which I have arrived, in pursuing it more closely, form the subject of the following section.

§ 3. *Influence of the permanent action of Magnetism on conducting bodies traversed by interrupted electric currents.*

Faraday's brilliant discovery of the action exerted by magnetism on a ray of polarized light, when that ray traverses a transparent body submitted to the action of a powerful electro-magnet, had no sooner been announced by its illustrious author, than the majority of philosophers saw in it a proof that magnetism, when at a high degree of intensity, has power to modify the molecular constitution of all bodies. They consequently attributed the phenomenon observed by Faraday, not to the direct action of the electro-magnet on the polarized ray, but to the modification effected by this action on the molecular constitution of the substance traversed by the ray. I was of this opinion, and communicated it to Mr. Faraday, who alludes to it in his memoir. Desirous, however, of founding this opinion on facts of a different kind, I asked myself if it were not possible to find in the electric current, an agent capable of performing the same function for opake conducting bodies that polarized light does for

transparent ones. I had stated in my paper on the sound emitted by iron wires traversed by interrupted electric currents, that the nature as well as the intensity of the sounds were singularly modified by the molecular state of the wire submitted to the experiment. I had particularly mentioned the influence of temper and annealing, of greater or less tension, and of temperature. I had shown that iron wire, when under the influence of an action which renders it magnetic, does not emit the same sound as when it is in its natural state. Finally, by modifying, through the agency of heat, the molecular arrangement of some metals, such as platinum and brass, I had succeeded in obtaining from them, during the passage of the interrupted current, sounds, which, though feeble, were yet distinct.

The preceding reflections tended to confirm me in my opinion, that sounds produced under the influence of magnetism in the experiments on the voltaic arc, are owing to a molecular modification effected by the action of the magnet, and the more so inasmuch as the voltaic arc may be regarded as produced by a succession of interrupted currents, following each other with extreme rapidity, rather than by a perfectly continuous current. I accordingly took bars of other metals besides iron, as of tin, zinc, lead, bismuth, &c. I placed them on the poles of the electro-magnet, and I caused an interrupted current from a Grove's battery of from five to ten pairs to traverse them. They emitted no sound as long as the electro-magnet was not magnetized, but as soon as it was, sounds were very distinctly heard, composed of a series of strokes corresponding to the interruptions of the current, and analogous to that produced by a toothed wheel. The bars were 18 inches long, and from 9 to 10 lines square. Rods of copper, platinum, and silver produced a similar effect; a rod of iron did not emit a much louder sound under the influence of the magnet than it did when not exposed to this action.

What appeared to me most remarkable was, to find lead, a body so little elastic, yield a sound as powerful as those proceeding from the other metals, when placed under the same circumstances. The position of metallic bars with respect to the poles of the electro-magnet did not in any way modify the result of the experiment; they might be placed axially, that is to say, in the direction of the poles, or equatorially, that is, across the poles; the effect remained the same, being merely weakened as the distance between the bar and the poles increased. In order to hear the sound distinctly, when not very powerful, it was sufficient to establish a communication between the metallic bar and the ear by means of a wooden rod. In this manner the sound was not unfrequently heard prolonged some seconds, though growing constantly feebler, until it ceased entirely, after the source of magnetism had been withdrawn from the electro-magnet. Mr. Faraday has remarked an analogous fact in the action of the transparent medium on the polarized ray, an action which does not cease immediately with the magnetism of the electro-magnet. Is this prolongation owing to the magnetization of the electro-magnet not ceasing in a sudden manner; or to its return to its primitive molecular state not taking place instantaneously in the

substance submitted to its action? This question I am unable to decide. I incline, however, rather to the latter of these explanations, seeing that the effect is not equally perceptible in all bodies, and that it is, for example, more sensible in a bar of bismuth than in one of copper.

It is needless to remark that the calorific action of the current could not have any influence on the production of the phenomenon, since there could have been no development of heat, on account of the dimension of the bars compared with the force of the current. Besides, if the expansion arising from the heating of the body traversed by interrupted currents had caused the sound, the effect would have been produced equally, whether the bar had been under the influence of the magnet or not. This last remark applies equally to the following experiments, as to the preceding.

The intensity of the sound appears to depend much less on the nature of the substance submitted to the experiment, than on its form, its volume, and its mass. Tubes of platinum, of copper and of zinc, emitted more marked sounds than massive cylinders of the same metals. I wound a leaden wire in the form of a helix round a cylinder of wood; I did the same with a very fine platinum wire, and also with copper, zinc, and tin wires, taking care to place the coils of the helices so far apart that each should be isolated. Placed like bars and tubes, whether in the direction of, or across the poles of the electro-magnet, these helices emitted very powerful sounds when, the electro-magnet being charged, they were traversed by the interrupted current. It was particularly surprising to hear the lead wire emit so strong a sound. A helix constructed with copper wire, covered with silk, and composed of several coils wound round each other, emitted a very intense sound; it also emitted one, but much feebler, under the action of the electro-magnet.

It is almost needless to remark, that in all these experiments an ordinary magnet produces the same effect as an electro-magnet. But what is more interesting, is to replace the action of the electro-magnet by that of a helix traversed by a strong continuous current, in the axis of which helix is placed the bar, the tube or the coiled wire, through which the interrupted current is transmitted. Experiments have shown me that in this case the results are the same; the intensity of the sounds is not very different, especially when tubes and wires coiled as helices are used.

If, between the exterior helix and the metal submitted to the action, a tube of soft iron is placed, the effect is a little heightened: it is neither increased nor lessened when the tube is of copper, only in this case another sound is heard which seems to proceed from the copper tube. This tube, however, is not traversed by a current, but it is probably acted upon by the currents of induction, which the interrupted currents traversing the conductor placed in the axis of the helix produce in it. I constructed a double helix formed of two thick copper wires covered with silk and coiled, each forming several circumvolutions, the one exterior to the other. In making a continuous current pass through the exterior wire, and an interrupted

current through the interior one, I heard a remarkably intense sound. In the reverse case, the sound existed, but was much weaker. This fact is evidently connected with the known property of helices traversed by electric currents exercising scarcely any magnetic influence exteriorly, whilst in the interior this action is very energetic.

Metals and solid bodies are not the only substances which produce the phenomena I have just described; all conducting bodies, whatever may be their state or their nature, appear to be capable of producing them. Thus, I have observed them with pieces of charcoal of all kinds and shape. Mercury also produces them in a very marked manner. I have inclosed mercury in a tube of glass an inch in diameter, and ten inches long: the tube was completely full and closed with care, so that the mercury could have no motion. As soon as it was traversed by an interrupted current, transmitted by means of two platinum wires, and the electro-magnet or the helix was made to act upon it, a sound was heard remarkable for its intensity. When the mercury was placed in an open trough, instead of being inclosed in a tube, it likewise produced a sound, and in addition there was seen on its surface an agitation or vibratory motion, very different from the gyratory motion observed by Davy, which appears under the influence of the poles of a magnet when traversed by a continuous current.

Dilute sulphuric acid, and what is even better, salt water, were successively put in a capsule of platinum placed on one of the poles of an electro-magnet. A point of platinum immersed in the liquid, served, together with the capsule, to send an interrupted current through it. A sound was again heard, but less distinct, on account of the noise produced by the disengagement of the gas: still it was so clear that no doubt could be entertained of its existence.

It may perhaps be thought that in the experiments I have just described, the sounds are produced by the mechanical action of attraction or repulsion exerted by the electro-magnet on the substance traversed by an interrupted current, and that, consequently, magnetism has no more share in the phenomenon than a finger might be supposed to have, when pressing on a sonorous cord. The simple description of the experiments shows this interpretation to be inadmissible. In the first place, the sound is the same with the wires in a helix, whether these wires be stretched or not, or whether they be of lead, platinum, or brass. Besides, how could this account for the sound produced in large masses, especially in liquids, such as mercury, and for the fact, that the position of the conductor traversed by the interrupted current with regard to the poles of the electro-magnet does not exert any influence on the phenomenon? Farther, it must be remarked that the sound in question is not a musical sound, such as would be produced by a string or mass made to vibrate by a cause acting exteriorly at its surface; it is a series of sounds corresponding exactly to the alternations of the passage of the current; like a species of collision of the particles amongst themselves. Thus, the phenomenon is molecular; and it leads to the demonstration of two important principles.

The first principle is, that the passage of the electric current modifies, even in solid bodies, the arrangement of the particles; a principle which I have already deduced from the experiments contained in my preceding memoir on this subject. The second principle is, that the action of magnetism, under whatever form it may be exerted, modifies alike the molecular constitution of all bodies, and that this modification lasts as long as the cause producing it endures, and only ceases with it. What is the nature of these two modifications? This is what we must endeavour to investigate and to ascertain. I purpose to engage in this inquiry, and indeed I have already made some attempts of which it would, however, be premature to give any account. I shall confine myself at present to a single remark, which does not appear to me to be devoid of interest: it is, that the influence of magnetism on all conducting bodies seems to impress on them, as long as it lasts, a molecular constitution similar to that which iron, and generally all bodies susceptible of magnetism possess naturally; for it developes in them the property of producing, when traversed by interrupted currents, sounds identical with those emitted also by iron and other magnetic bodies when transmitting these currents, but produced in these last without requiring the action of a magnet.

V. *On the Lunar Atmospheric Tide at St. Helena.*

By Lieut.-Colonel Edward Sabine, *R.A., For. Sec. R.S.*

Received December 16, 1846,—Read January 28, 1847.

THE attractions of the sun and moon occasion tides in the atmosphere similar to those of the ocean. The effects are, however, so inconsiderable in comparison with the disturbances produced in the equilibrium of the atmosphere by other causes, that hitherto observation has not succeeded in affording any clear or decisive evidence of them. The phenomenon however has a very considerable philosophical interest as a farther exemplification of the universality of the principle of gravitation.

As the ebb and flow of the atmospheric tide must be greatest in the vicinity of the equator, and as moreover the ordinary equilibrium is less disturbed by irregular causes in that region of the globe than in the temperate zones, it was reasonable to expect that the existence of a tide in the atmosphere, depending on the position of the sun or moon relatively to the meridian of the place, would be shown, if at all, by observations made within the tropics; and that the most favourable situation for such observations might be that of a small island surrounded by a considerable extent of ocean, and therefore comparatively free from the atmospheric disturbances occasioned by variations of the terrestrial surface; and where also the tides of the ocean should be small.

The importance of St. Helena as a station of observation for this purpose early engaged the attention of the highly intelligent director of its magnetical and meteorological observatory, Captain Lefroy of the Royal Artillery, as may be seen from the following extract of a report received from that officer, dated June 1st, 1842.

"I believe that no observations have as yet decided the question, whether any effect upon the mean barometrical pressure is produced by the moon's daily passage of the meridian. The existence of an atmospheric tide of this nature appeared however so interesting a subject of inquiry, and its detection so probable, owing to the extreme uniformity of the daily oscillation, that the observations of seventeen months, viz. from August 1840 to December 1841 inclusive, have been arranged with that view. The mode adopted was this:—the corrected height of the barometer (*i. e.* the reading reduced to 32° Fahr.) at the hour of observation nearest the moon's meridian passage for every day has been entered in a central column; and in parallel columns headed -2^h, -4^h, &c. and $+2^h$, $+4^h$, &c. have been entered the observations taken respectively at 2^h, 4^h, &c. before and after the central observation. A mean has been taken for the observations included in each lunar month. It appears from the

seventeen months thus examined, that a maximum of pressure corresponds to the moon's passage over both the inferior and superior meridians, being slightly greater in the latter case; and that a minimum corresponds nearly to the rising and setting, or to $\pm$ 6h. The average of the seventeen months gives the respective pressures as follows, viz.—

	in.
Moon on the meridian	28·2714
Moon in the horizon	28·2675

The difference being ·0039 in.

"The latitude of the observatory is $-15° 57'$; the height above the sea, ascertained by levelling, 1764 feet to the cistern of the barometer. Observations made in 1827 under the direction of Major-General Walker, gave the particulars of the oceanic tides as follows:—

Rise at new moon . .	3 feet 6 in.
Rise at full moon . . .	2 feet $9\frac{1}{2}$ in.
Rise at the quarters . .	1 foot 5 in.
Establishment	2h, 20m.

"There appears to be no establishment in the atmospheric tide, consequently the rise in the ocean will not account for the variation in the height of the barometer, because the times of maximum do not coincide."

Early in 1842 Captain Lefroy was succeeded in the charge of the observatory at St. Helena by Captain Smythe of the Royal Artillery; and from the 1st of October of that year, the observations, which up to that period had been taken at every second hour of mean solar time, were taken at every hour, and thus in their re-arrangement in lunar time, the mean height of the barometer at the different lunar hours became better represented than had been the case under the two-hourly system. During the first year of the hourly observations, viz. from October 1842 to September 1843 inclusive, the examination of the moon's influence continued to be carried on at the observatory by Captain Smythe in a somewhat modified manner, which is thus described by him:—"The hourly observations for each day, extracted from the day-book, were grouped into lunar months, and the monthly mean for each hour found. The observation taken each day at the hour nearest the time of the moon's meridian transit was inserted in the centre column for that day, and the other observations disposed right and left in order. As however the observations in this state are affected by the diurnal variation, which at St. Helena is very regular and considerable, and by which the moon's effect would have been overridden, the monthly mean for the hour was subtracted from each observation taken at that hour, and the remainder regarded as due to the moon's action. When only twenty-three observations intervened between two meridian transits of the moon, the middle observation was entered in both the -12 and $+12$ column."

Variation of the Barometer at the several Lunar Hours, from October 1842 to September 1843 inclusive, in decimals of an inch, + signifying an excess of barometric pressure, and — a defect.

1st. From the superior to the inferior passage.

Moon on the upper meridian.	+1h.	+2h.	+3h.	+4h.	+5h.	+6h.	+7h.	+8h.	+9h.	+10h.	+11h.	+12h.
+·0014	+·0011	+·0002	·0000	—·0008	—·0010	—·0012	—·0018	—·0011	—·0005	+·0002	+·0012	+·0012

2nd. From the inferior to the superior passage.

—12h.	—11h.	—10h.	—9h.	—8h.	—7h.	—6h.	—5h.	—4h.	—3h.	—2h.	—1h.	Moon on the upper meridian.
+·0016	+·0012	+·0009	+·0002	—·0006	—·0010	—·0011	—·0010	—·0004	+·0004	+·0003	+·0016	+·0014

We have here an average excess of barometric pressure of ·0014 in. at the hour when the moon is on the meridian above or below the pole, and an average defect of ·00115 at the period when she is six hours distant from the meridian; making together an average difference in the height of the barometer of ·00255 in. between the hours when the moon is on the meridian and when she is six hours distant from it.

The arrangement of the observations for the investigation proving rather a heavy charge on the establishment at St. Helena, the examination was now taken up at Woolwich, and carried through a subsequent period of two years, *i. e.* from October 1843 to September 1845 inclusive, for which the following method was adopted:—

If we call b the height of the barometer at 32° at any hour of observation, and $\overline{b}$ the mean height of the barometer at the same hour during the month to which the day belongs, then $b-\overline{b}$ is a quantity which remains over after the approximate diurnal variation has been eliminated; it is + when the barometer is higher and — when it is lower than the normal state, and expresses by how much the barometer is higher or lower at that observation than the normal state at the same hour. Monthly tables are then formed in which are inserted the values of $b-\overline{b}$ for each hour of mean solar time. The mean solar hours which are respectively nearest to the several lunar hours are then computed for every day, and the values of $b-\overline{b}$ are re-arranged in lunar monthly tables. The means at the several lunar hours in each month are then taken; and these means are finally arranged in periods of six months, yielding mean values of the barometrical variation at the several lunar hours for each half year. These are shown in the following tables:—

1st. From the superior to the inferior passage.

	Lunar hours.											
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Oct. 1843 to March 1844 ...	+·0017	+·0012	+·0004	−·0007	−·0021	−·0018	−·0018	−·0016	−·0019	−·0013	−·0004	+·0014
April 1844 to Sept. 1844 ...	+·0023	+·0020	+·0015	+·0007	−·0001	−·0013	−·0028	−·0027	−·0011	−·0004	−·0006	+·0009
Oct. 1844 to March 1845 ...	+·0021	+·0026	+·0023	+·0014	+·0007	+·0001	−·0007	−·0003	+·0005	+·0010	+·0017	+·0006
April 1845 to Sept. 1845 ...	+·0009	+·0001	−·0008	−·0018	−·0021	−·0024	−·0032	−·0024	−·0008	+·0008	+·0016	+·0026
Mean of the 1st year.........	+·0020	+·0016	+·0009	·0000	−·0011	−·0016	−·0023	−·0022	−·0015	−·0009	−·0005	+·0012
Mean of the 2nd year	+·0015	+·0014	+·0008	−·0002	−·0007	−·0012	−·0020	−·0014	−·0002	+·0019	+·0016	+·0016
Mean of the four half years	+·00175	+·00148	+·00085	−·00010	−·00090	−·00135	−·00212	−·00175	−·00082	+·00002	+·00058	+·00138

2nd. From the inferior to the superior passage.

	Lunar hours.											
	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.
Oct. 1843 to March 1844 ...	+·0021	+·0020	+·0015	−·0006	−·0002	−·0005	−·0008	−·0007	+·0003	+·0012	+·0016	+·0018
April 1844 to Sept. 1844 ...	+·0005	−·0002	−·0007	−·0009	−·0015	−·0014	−·0015	−·0005	+·0003	+·0018	+·0024	+·0027
Oct. 1844 to March 1845 ...	+·0011	+·0007	−·0004	−·0024	−·0024	−·0026	−·0026	−·0023	−·0018	+·0001	·0000	+·0005
April 1845 to Sept. 1845 ...	+·0027	+·0022	+·0018	+·0010	−·0006	−·0015	−·0024	−·0023	−·0021	−·0001	+·0006	+·0012
Mean of the 1st year.........	+·0013	+·0009	+·0004	−·0008	−·0009	−·0010	−·0012	−·0006	+·0003	+·0015	+·0020	+·0023
Mean of the 2nd year	+·0019	+·0015	+·0007	−·0007	−·0015	−·0021	−·0025	−·0023	−·0020	·0000	+·0003	+·0009
Mean of the four half years	+·00160	+·00118	+·00055	−·00072	−·00118	−·00150	−·00182	−·00145	−·00082	−·00075	+·00115	+·00155

The evidence is here of the most decisive character of a barometrical maximum at the lunar hours of 0 and 12, and a minimum at 6 and 18, with a corresponding progression at the intermediate hours.

If we now arrange these results in such manner that the hours are combined in which the moon is similarly situated in respect to the meridian, we have the lunar horary variation of the barometer as follows:—

Moon's distance from the meridian.	Variations of barometric pressure.				Horary variation.			Moon's distance from the meridian.
	At the hours following the meridian passage.		At the hours preceding the meridian passage.		From the observations at the hours following the meridian passage.	From the observations at the hours preceding the meridian passage.	Mean.	
h	h in.	in.	h in.	in.	in.	in.	in.	h
0	0+·00175 12+·00160	+·00168	0+·00175 12+·00160	+·00168	+·00365	+·00365	+·00365	0
1	1+·00148 13+·00118	+·00133	11+·00138 23+·00155	+·00146	+·00330	+·00343	+·00336	1
2	2+·00085 14+·00055	+·00070	10+·00058 22+·00115	+·00086	+·00267	+·00283	+·00275	2
3	3−·00010 15−·00072	−·00041	9+·00002 21−·00075	−·00036	+·00156	+·00161	+·00158	3
4	4−·00090 16−·00118	−·00104	8−·00058 20−·00082	−·00070	+·00093	+·00127	+·00110	4
5	5−·00135 17−·00150	−·00142	7−·00175 19−·00145	−·00160	+·00055	+·00037	+·00046	5
6	6−·00212 18−·00182	−·00197	6−·00212 18−·00182	−·00197	00000	00000	00000	6

It should here be remarked, that as the observations were made at hours of solar time, they could not of course strictly correspond in the majority of cases to the lunar hours under which the value of $b-\overline{b}$ have been arranged. The effects of this imperfect synchronism have a tendency to compensate each other, and are probably nearly compensated, at all the lunar hours except at 0 and 12, 6 and 18. These, however, are the most important hours for the determination of the maximum effect, and, supposing there to be no establishment, it is obvious that whether the observation precedes, or whether it follows the precise lunar hour to which it should correspond, the error produced will be the same in kind, viz. the maximum at 0^h and 12^h will be lowered, and the minimum at 6^h and 18^h will be raised; and in both cases the error will tend to diminish the apparent influence of the moon's position on the barometric pressure. It may therefore be presumed that the true horary variation corresponding to the lunar hours is in every case greater than the numbers which appear in the preceding table; and that the horary variation at 0^h and 12^h which should express the whole difference of pressure corresponding to the moon's positions on the meridian and at six hours' distance from it, is especially less than the true amount, being diminished by the causes above mentioned, both at the hours when the moon is on the meridian and when she is six hours distant. To obviate this inconvenience and to give the results in future as much precision as they are capable of, eight additional observations, at equal lunar intervals, will hereafter be made in each day, corresponding precisely to the lunar hours of 0, 3, 6, 9 . . . . . 21; and these observations will be made on Sundays as well as on other days.

We may conclude therefore as the result of the two years of observation from October 1843 to September 1845 inclusive, that the barometer at the Observatory at St. Helena is higher when the moon is on the meridian, either above or below the pole, than when she is six hours distant from the meridian, by an average quantity which exceeds ·00365 in., and may be taken in round numbers as ·004 in.

For the purpose of examining whether any perceptible difference in the influence of the moon on the barometric pressure could be detected at the periods of the apogee and perigee, the following method was adopted. The epoch of the moon's perigee or apogee being taken from the Nautical Almanac, the nearest of the lunar hours 0, 6, 12, or 18 to that epoch is taken as the middle term of comparison; if it be 0 or 12 hours, the values of $b-\overline{b}$ at the four antecedent and four subsequent lunar meridian hours (0 and 12) are taken in addition to the middle term, to give a mean value corresponding to the times when the moon is on the meridian above or below the pole. The mean of the eight intermediate values of $b-\overline{b}$ at 6 and 18 hours taken in like manner, furnishes a mean value corresponding to the times when the moon is six hours distant from the meridian. The difference between these mean values of $b-\overline{b}$ gives for the epoch in question, the excess of the barometrical pressure when the moon is on the meridian above the pressure when she is six hours distant from it. Theoretically, this excess should be greater at periods of perigee than at those of apogee.

If the lunar hour nearest to the epoch of apogee or perigee be 6 or 18 instead of 0 or 12, that hour is taken as the middle term of comparison, and the mean values of $b-\bar{b}$ consist in such case of the mean of nine values at 6 and 18 hours, and of eight at 0 and 12. When a Sunday intervenes the same number of values of $b-\bar{b}$, viz. eight of the one and nine of the other, are taken to give the mean value in the comparison, which consequently in such case extends to a somewhat greater distance on either side of the middle term than when no such interruption occurs.

The mean excess of barometrical pressure when the moon is on the meridian, resulting from this comparison, is as follows :—

From 13 epochs of perigee between October 1843 and September 1844 ·00407 in.
From 13 epochs of perigee between October 1844 and September 1845 ·00394 in.
From 13 epochs of apogee between October 1843 and September 1844 ·00341 in.
From 14 epochs of apogee between October 1844 and September 1845 ·00347 in.

The number of observations from which each of the first three results is obtained is 221 ; in the fourth case it is 238.

We have here in both years a consistent indication of the greater influence of the moon on the barometrical pressure at periods of perigee than at those of apogee.

The effect of the solar action on the atmospheric pressure is far more difficult to be shown by any mode of grouping the observations than is that of the moon, not only because it is much smaller, but because it is masked in the diurnal barometric oscillation by the much greater and variable influence of the solar heat in producing atmospheric variations. That it is eliminated, in common with the other variations which depend on the sun's horary angle, by the process described in this paper, may be inferred from the fact that no significant difference appears in comparing the excess in the values of $b-\bar{b}$ at the lunar hours of 0 and 12, at the periods of syzygies and quadratures, during the twelve months from October 1843 to September 1844 inclusive. The mean values in this comparison have been taken from the observations at 0^h and 12, 6 and 18 for 36 hours preceding, and 36 hours following, the epoch of each syzygy and quadrature. The mean excess at 0 and 12 at the periods of syzygy is ·00337, and at those of quadrature ·00345.

The quantities treated of in this communication will justly appear to many persons as extremely small; but the consistency of the partial results, when the observations are broken into periods of six months, places beyond doubt the power of observation, in appropriate circumstances and continued for a sufficient time, to determine both the existence and the approximate amount of a diurnal systematic affection of the barometer, not exceeding four thousandths of an inch. It also affords an instructive example of the beneficial influence of *mean numerical values* in the advancement of the physical sciences, and of the power which we possess through their means of progressively separating from apparently irregular phenomena, that which is constant and reducible to laws.

VI. *On the Diurnal Variation of the Magnetic Declination at St. Helena.* *By Lieut.-Colonel* Edward Sabine, *R.A., For. Sec. R.S.*

Received January 21,—Read February 18, 1847.

It has long been known that in Europe the north end of a magnet suspended horizontally (meaning by the north end the end which is directed towards the north), moves to the *East* from the night until between 7 and 8 o'clock in the morning, when an opposite movement commences, and the north end of the magnet moves to the *West*. Recent observations have shown that a similar movement takes place at the same hours of local time in North America, and that it is general in the middle latitudes of the northern hemisphere.

It has also been known for some years past, and has been confirmed by recent observations, that in the middle latitudes of the southern hemisphere, the north end of the magnet moves in a contrary direction to that which has been described as taking place in the northern hemisphere, viz. that it moves to the west until 8 o'clock in the morning, or thereabouts, and then returns towards the east.

From the contrariety of the movement which is thus found to take place in the same meridians at the same hours in the opposite hemispheres, it seemed a not unreasonable conjecture, that at some intermediate point in each meridian, the causes, whatever they might be, which occasion these movements might counterbalance each other, and that the diurnal variation might consequently disappear: and questions were raised whether the line connecting these points in the different meridians would be found to coincide with the terrestrial equator, or with the line of no dip, or with one of the isodynamic lines.

The problem, which observation was thus called upon to resolve, has been stated with so much perspicuity by M. Arago in the Annuaire for 1836, that I may be permitted to reproduce it in his own words:—

" Dans l'hémisphère nord, la pointe d'une aiguille horizontale aimantée, qui se tourne vers le nord, marche, de l'est à l'ouest, depuis $8\frac{1}{4}^{h}$ du mat. jusqu'à $1\frac{1}{4}^{h}$ après midi; de l'ouest à l'est, depuis $1\frac{1}{4}^{h}$ après midi jusqu'au lendemain matin. Dans l'hémisphère sud, la pointe tournée vers le nord marche, de l'ouest à l'est, depuis $8\frac{1}{4}^{h}$ du matin jusqu'à $1\frac{1}{4}^{h}$ après midi; c'est précisement l'opposé du mouvement qu'effectue, aux mêmes heures, dans notre hémisphère, la même pointe nord.

" Supposons qu'un observateur partant de Paris s'avance vers l'équateur. Tant qu'il sera dans notre hémisphère, la pointe nord de son aiguille effectuera tous les matins un mouvement vers l'occident; dans l'hémisphère opposé, la pointe nord de cette

même aiguille éprouvera tous les matins un mouvement vers l'orient. Il est impossible que ce passage du mouvement occidental au mouvement oriental se fasse d'une manière brusque : il y a nécessairement entre la zone ou s'observe le premier de ces mouvements, et celle où s'opère le second, une ligne où, le matin, l'aiguille ne marche ni à l'orient ni à l'occident, c'est-à-dire reste stationnaire.

"Une semblable ligne ne peut pas manquer d'exister ; mais où la trouver ? Est-elle l'équateur magnétique, l'équateur terrestre, ou bien quelque courbe d'égale intensité?"

In the recent work of the Baron von HUMBOLDT (Cosmos, vol. i.), this question is also adverted to, and the problem is stated in nearly similar terms : after noticing the contrariety of movement in the two hemispheres, Baron von HUMBOLDT remarks that attention has been justly called to the belief "that there must be a region of the earth, probably between the terrestrial and magnetical equators, in which no horary variation of the declination is sensible. This fourth curve, which might be called the curve of no motion, or rather *the line of no horary variation of the declination*, has not yet been found."

In the choice of the stations at which the magnetical observatories established by the British Government and by the East India Company in 1840 were placed, the solution of this problem was not overlooked. SINGAPORE is situated close to the terrestrial equator ; it was therefore well-suited to meet the suggestion of M. ARAGO in that respect : it is also not far distant from the line of no dip, and might be expected therefore to exhibit, in some degree at least, the peculiarities which might appertain to stations so circumstanced. In this country, a somewhat different mode of viewing the magnetic system of the globe from that which prevailed generally in France, caused an opinion to be entertained, that a different line from any of those suggested by M. ARAGO might not improbably prove the dividing line of the two magnetic hemispheres in this respect ; and that the phenomena of the diurnal variation, whatever they may be, which should characterise the dividing line, might be most advantageously studied at a station chosen in its vicinity. The line here referred to passes round the globe, crossing the several meridian lines at points where the magnetic intensity in each is a minimum : or, it may be more precisely defined, as the locus of the points of minimum intensity of all lines on the surface of the earth drawn at right angles to itself. Its position has been traced through all the meridians of the globe with considerable approximation*. It is not by its definition necessarily an *isodynamic* line, or a line of equal magnetic force ; and in fact, it is far from being so, the intensity of the force ranging in different parts of the line from 6·4 to 7·6 in absolute measure. It happens that Singapore, which, as already stated, is situated close to the terrestrial equator, and near the line of no dip, is in a part of the globe where the lines of least force and of no dip approach each other most nearly ; consequently the observations at Singapore might be expected to exemplify the

* Reports of the British Association, 1837. Philosophical Magazine, vol. xiv. p. 81.

phenomena, whatever they might be, of an intermediate station, whether the intermediate character should be derived from proximity to the terrestrial equator, to the line of no dip, or to that of least force. St. HELENA is situated close to the line of least magnetic force in a quarter of the globe where that line departs most widely both from the terrestrial equator and from the line of no dip; its latitude being about -16° and the dip about -22°. Should therefore the diurnal variation at St. Helena be found to possess the intermediate character, it was considered that it would go far to indicate that the character was given by proximity to the line of least intensity, rather than to either of the two other lines. A third station, the CAPE OF GOOD HOPE, seemed well-suited to subject this latter point to a still severer scrutiny; although somewhat more distant than St. Helena from the line of least intensity (which passes between those stations but nearer to St. Helena than to the Cape), the magnetic force at the Cape is still so weak as not to exceed in absolute measure the intensity on some parts of the line of least force (in the neighbourhood of Singapore for example): it was considered therefore as not improbable that if the intermediate character should prove to belong to stations at or near the line of least intensity, the Cape of Good Hope might be found to partake of the peculiarities of such a station, although the distance of the Cape from the terrestrial equator is not less than 34°, and the dip exceeds -53°.

From the moment when the observations of the first complete year at St. Helena and the Cape arrived in England and were examined, their bearing on the solution of the problem was perceived: but as opinions had been expressed of the probable influence of the cylindrical boxes in which the magnetometers were originally placed in generating currents of air at particular hours of the day and seasons of the year, and possibly of vitiating to a greater or less degree the diurnal variation thus observed, it was judged more prudent to suspend a notice of the inferences to which they led, until the observations of subsequent years made with additional precautions should have been received.

The results which will be now communicated to the Society, are founded on the observations at St. Helena of five years' continuance, viz. from 1841 to 1845 inclusive. From the beginning of 1841 to July 1843, the magnet was inclosed in a cylindrical box, corresponding to the description in page 14 of the Report of the Committee of Physics of the Royal Society; and from July 1843 to the end of 1845, in a double rectangular casing, which is thus described in a note from Captain SMYTHE of the Royal Artillery, Director of the St. Helena Observatory:—

"A double rectangular box was made to inclose the magnet and replace the cylindrical one with glass top hitherto used. It is composed of two boxes made of mahogany, every way similar to each other, the outer one being about an inch in all its dimensions larger than the inner one. The boxes are divided in the middle, and the halves fit close when pushed together by being half sunk into each other where they join. The interior of the inner box, and the exterior of the outer, are covered

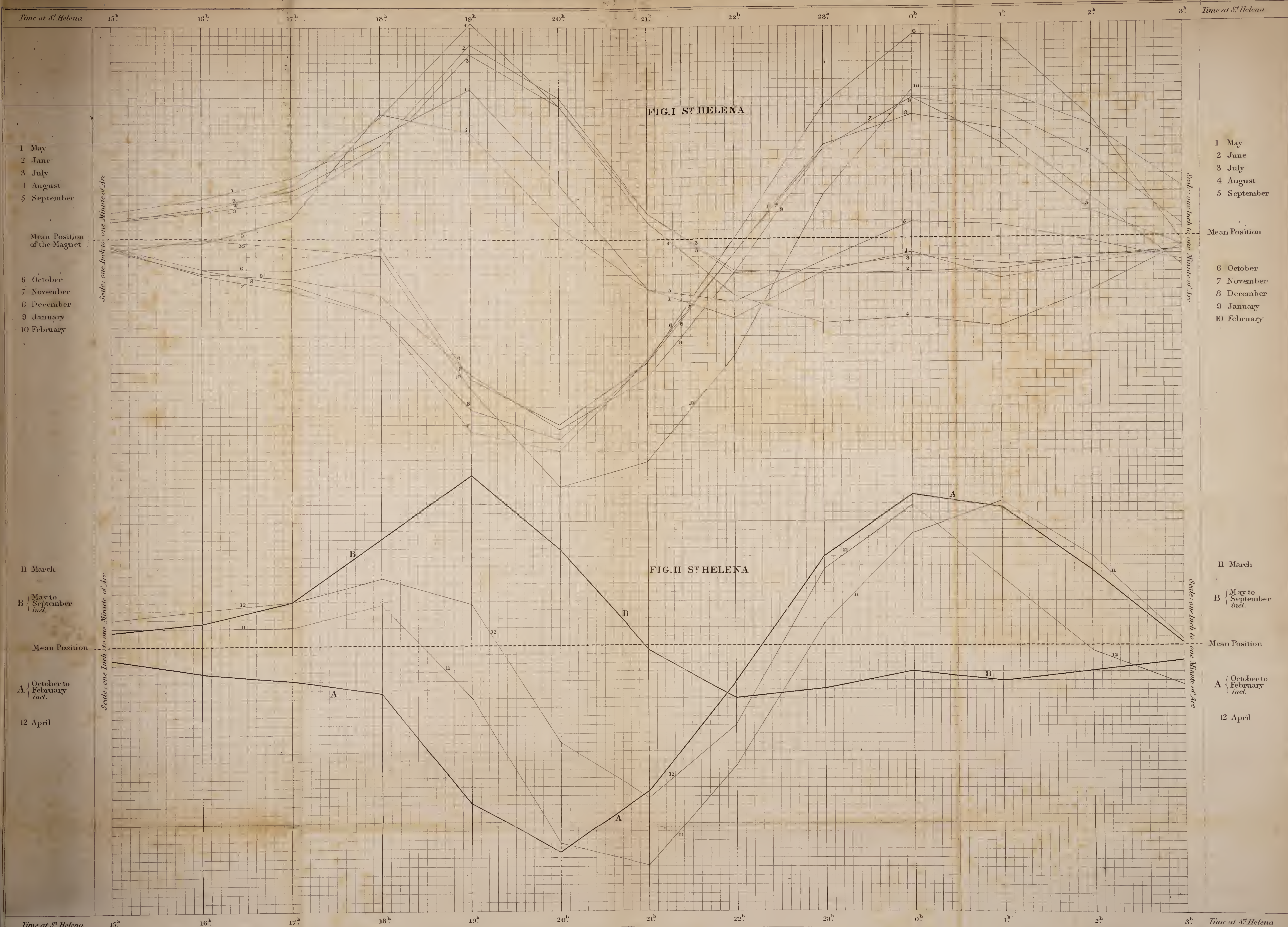
with gilt paper, and the boxes are kept firm in their place by screws which pass through the cross piece connecting the copper pillars, and resting upon the top of the outer box."

On a careful comparison of the observations before and after this change was made, no material difference is perceived which can be ascribed to currents of air produced by either form of the magnetometer box. The law of the diurnal variation is the same whether it be deduced from the observations of the first period of $2\frac{1}{2}$ years, or from the second period of the same duration. The extent of the arc which the magnet passes through in the twenty-four hours as a consequence of the diurnal variation, whether it be measured by the difference of the extreme east and west positions of the magnet, or by the sum of the fluctuations observed from hour to hour, is not the same in different months of the year, and is also found to differ to a small amount in the same months in different years: but these are obviously real differences depending probably on occasional inequalities in their magnetic causes, and appear equally in either series considered separately. The law of the variation in each month may generally be derived from the observations of any one year of the series, but its average amount is more correctly derived from a mean of several years. I have therefore employed in this communication the mean of the five years, but without burthening the paper by inserting the details of the separate series.

The diurnal variation which we obtain from the observations of the five years at St. Helena. shows that that station was well-chosen for the purpose of contributing to the solution of the problem in question: but the solution to which it conducts is of a very different character from that which was anticipated, and is one which seems not unlikely to assist materially in the eventual elucidation of the physical causes of the periodical variations. We have seen that in the northern portion of the globe the magnet moves to the east until seven or eight o'clock in the morning, and then returns to the west; it does so in every month of the year; the extent of the movement is greater in the summer than in the winter months, but the direction is always the same. So in the southern portion of the globe, the movement in the contrary direction is also constant throughout the year; it is greater when the sun is in the southern signs than when he is in the northern signs, but the direction is the same in all the months of the year; an extreme of westerly elongation is reached in every month about the hour of 8 A.M., as is an extreme of easterly elongation about the same hour in the opposite hemisphere. At St. Helena the well-marked peculiarity of the diurnal variation is, that during one-half of the year the movement of the north end of the magnet at the hours above referred to corresponds in direction with the movement which is taking place in the northern hemisphere, whilst in the other half of the year the direction corresponds with that which is taking place in the southern hemisphere. The opposite movements which take place simultaneously in every day of the year in the same meridians in the two hemispheres, do not by their mutual opposition neutralise each other and thus leave the magnet stationary. On the con-

DIURNAL VARIATION OF THE MAGNETIC DECLINATION

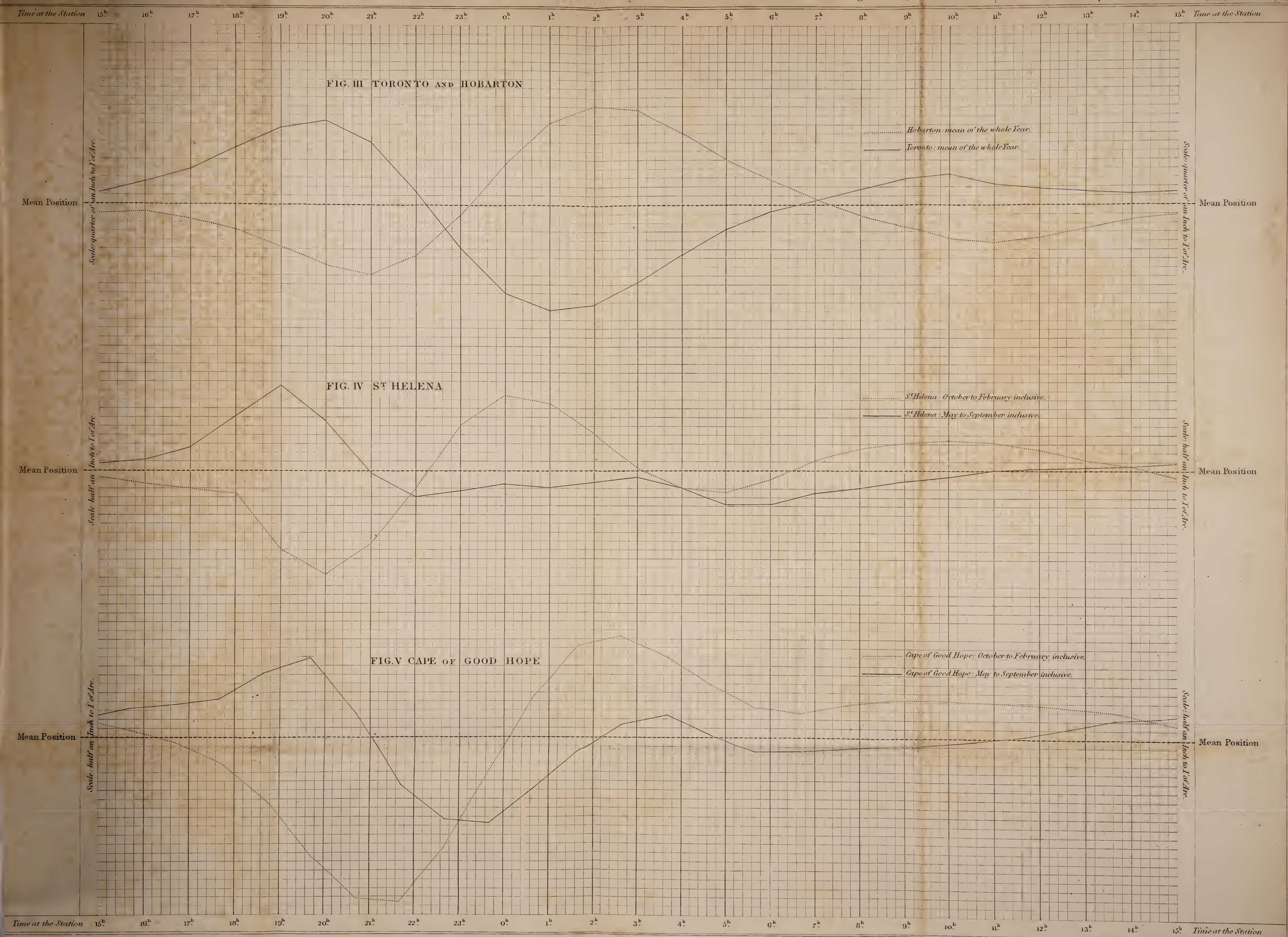
The NORTH end of the magnet moving towards the EAST, ↑ towards the WEST. ↓



J. & C. Walker Sculp.t

DIURNAL VARIATION OF THE MAGNETIC DECLINATION

The NORTH end of the magnet moving towards the EAST; towards the WEST.



J.&C.Walker Sculp!

trary, the diurnal variation at St. Helena partakes in, and possesses the characters of the phenomena of both hemispheres, but each predominates in its turn, prevailing separately and in opposite seasons. The passage from one order of the phenomena to the other takes place at or soon after the period of the equinoxes; in March and April, September and October, the diurnal variation at the hours referred to partakes, more or less on different days, of the characteristics of both seasons: but the months of May, June, July and August, on the one hand, and those of November, December, January and February, on the other, arrange themselves in wholly distinct categories; the north end of the magnet reaching its eastern extreme in the one case, and its western extreme in the other, nearly at the same hours; the extremes being moreover in both cases nearly equidistant from the mean position of the magnet in the respective months.

In Plate III. figs. 1 and 2 exhibit the projections of the diurnal variation at St. Helena from 3 A.M. to 3 P.M., in each month of the year, the projections representing the mean of five years of observations. In fig. 1, which contains all the months excepting March and April, September is seen to belong on the whole to May, June, July and August, although the influence of the opposite season is plainly visible at the hours of 18 and 19 (6 and 7 A.M.). October, on the other hand, must be classed with November, December, January and February, although an influence of opposite character is distinctly perceptible in the direction from 17^h to 18^h (5 to 6 A.M.)*. In fig. 2, the projections corresponding to the months when the sun has northern declination are collected into one darker line, as are those of the other five months when the sun has southern declination into another, for the purpose of exhibiting by comparison with the separate projections for March and April (which are the fainter lines), the degree to which the latter are intermediate. March and April have each both an eastern and a western elongation in the early morning hours; the eastern occurs one hour earlier than in May to September, and the western one hour later than in October to February: these peculiarities, both in regard to direction and to hours, are traceable without difficulty to the occasional alternation in those months of the influences of the opposite seasons.

Fig. 3. Plate IV. exhibits in the unbroken line the mean diurnal variation of the whole year at Toronto, and in the broken line the mean variation of the whole year at Hobarton in Van Diemen Island, showing the contrast which the *opposite hemispheres* present in this respect. In like manner the broken and unbroken lines in fig. 4 present in contrast with each other the diurnal variation of the *opposite seasons* at St. Helena. By the comparison of figs. 3 and 4, the general resemblance is shown

* The fact that October corresponds much more to November, December, January, and February than to May, June, July, and August is very decided; and is important to notice, because, although the sun passes to the south of the equator in September, he continues to the north of the parallel of St. Helena until the beginning of November. It is the sun's position in reference to the earth's equator, therefore, and not to the zenith of the place of observation, which marks the epoch of the change in the direction of the diurnal variation of the needle.

between the effect of the opposite hemispheres in the one case, and of the opposite seasons in the other, whilst at the same time the subordinate points of dissimilarity are equally conspicuous. It is not the object of this paper to enter on the discussion of minor points of difference, such as the non-agreement of the turning hours, which are somewhat earlier at St. Helena than at Toronto in the one case and at Hobarton in the other, and the check which appears to take place in the western elongation in May and September at St. Helena towards the hour of noon; but I may permit myself to notice, as a minor but apparently characteristic point of resemblance, the circumstance that the eastern elongation at the morning hour at Toronto, as well as in the corresponding season at St. Helena, always precedes by an hour the western elongation about the same period of the day at Hobarton and its corresponding season at St. Helena. This feature has a further tendency to connect a hemispherical peculiarity of daily occurrence throughout the year in each of the two hemispheres, with a periodical peculiarity at St. Helena conforming strictly to the alternation of the seasons.

The projections for Toronto and Hobarton in fig. 3 represent each a mean of two years of hourly observations; the scale on which they are drawn has been taken for convenience at half the magnitude of the scale on which the St. Helena projections in fig. 4 are drawn.

We have hitherto considered those portions of the diurnal variation at St. Helena which include the hours of the forenoon and of the first part of the afternoon. We have seen that from 3 A.M. to 3 P.M. the movement of the magnet is strikingly dissimilar, and is even opposite for a considerable portion of those hours, in the two solstitial periods of the year. A less marked but not less systematic difference takes place during the remaining hours, as is shown by the projections in fig. 4, which correspond respectively to the months from May to September, and from October to February. The diurnal variation at these hours, from May to September, consists in a small but continuous and steady motion of the north end of the magnet towards the east, commencing at 5 or 6 P.M., and continuing without interruption through the night until the following morning: whereas from October to February, the motion, which at first is in the same direction, is more considerable, and an inferior eastern extreme is reached about 9 or 10 P.M., to which there is nothing analogous at the other period of the year; a return then takes place towards the west (contrary to the direction in the opposite season), and is thenceforward continuous until the forenoon of the next day. In the night portion therefore of the diurnal curve as well as in that portion which has been more largely discussed, the horary variation at St. Helena does not disappear, but continues to exhibit a diversity at opposite seasons, in which an analogy may still be traced to the difference in the annual projections at Toronto and Hobarton at the same hours, shown in fig. 3. It should be noticed however that the correspondence which exists during the hours of the day between Toronto and those months at St. Helena which form the northern summer on the

one hand, and Hobarton and those months at St. Helena which form the southern summer, is not preserved in the night portion of the curve.

If the phenomena of the diurnal variation at St. Helena are characteristic of a station situated between the northern and southern magnetic hemispheres, partaking, although in opposite seasons, of those contrary features which separately prevail in the two hemispheres throughout the year, then we must regard the diurnal variation at the Cape of Good Hope, notwithstanding the remoteness of the situation of the Cape from the terrestrial equator and from the line of no dip, as supplying an additional illustration of the phenomena of an intermediate station. The projections in fig. 5, corresponding to the seasons May to September, and October to February, each representing also the mean of five years of hourly observation, bears a striking general resemblance to those of the preceding figure; the principal minor modifications also are such as may readily be imagined to be occasioned by the greater distance of the Cape from the dividing line. It will be remarked that the contrary movements at the opposite season of the year take place at the Cape of Good Hope as well as at St. Helena, although the sun is throughout the year to the north of the parallel of the Cape, and consequently is always north of its zenith.

Having named Singapore at the commencement of this paper, I may add that I have examined the manuscript observations of that station in 1841 and 1842, which are at the Royal Society: the observations appear to have been taken in those years at every second hour only, consequently the diurnal variation derivable from them is less complete than from an hourly record, but is quite sufficient to show that in general characters it corresponds with that of St. Helena and the Cape of Good Hope: the movement of the north end of the magnet in the months of May, June, July and August, is to the east until 7 or 8 A.M., followed by an immediate return to the west; and in November, December, January and February, to the west until 8 A.M., followed by an immediate return to the east: the last-named months have also an easterly extreme about 2 P.M., which has no similar or opposite feature in May, June, July and August.

From the facts here brought forward, it may be inferred that the line which has been supposed to exist by the eminent authorities referred to in the commencement of this paper, and which should be characterized by the absence of a diurnal variation of the declination, will not be found upon the globe.

VII. *On the Automatic Registration of Magnetometers, and other Meteorological Instruments, by Photography.* By Charles Brooke, *M.B., F.R.C.S.E. Communicated by* G. B. Airy, *Esq., F.R.S., Astronomer Royal.*

Received June 16,—Read June 18, 1846.

An efficient method of continuously registering the variations of magnetometers having been generally admitted to be a desideratum in science at the Magnetic Conference held during the Meeting of the British Association at Cambridge, it became a matter of philosophical interest to supply the deficiency, and this, it is hoped, has by the following means been satisfactorily accomplished.

As the mechanical force which a suspended bar magnet is capable of exerting, during its variations of direction, is far too minute to actuate the most delicate mechanism without sensibly affecting its position, the desired object will probably be obtained by photography alone. In order to render any method of photographic registration practically useful, it is essential that the three following indications should be fulfilled.

First, to obtain an easily managed artificial light of sufficient intensity to affect photographic paper, especially at those periods when it is of most consequence to obtain a continuous register, namely, when the position of the magnet is undergoing great and rapid variations.

Secondly, to prepare by a ready process photographic paper sufficiently sensitive to receive the feeble impressions of artificial light, and at the same time sufficiently durable to retain those impressions during a period of at least twelve hours, as a more frequent attention to the apparatus would probably interfere with the ordinary arrangements of an observatory.

Thirdly, to magnify the movements of the magnet by some optical arrangement, so that the variations may be indicated with sufficient minuteness and accuracy.

The first point may be attained by a camphine lamp, the light of which is much whiter and more intense than that of any other known lamp; that which has been in use (see fig. 3, Plate V.) is of a square form, each side being $2\frac{1}{2}$ inches long and $1\frac{1}{2}$ high, and stands in a little wooden frame made exactly to fit it, that it may with certainty be replaced in exactly the same position, if removed for the purpose of being trimmed. Many registers prove this to have been the case, there not having been any visible displacement of the line at the recorded period of removal of the lamp. The wick is flat, and about 0·5 inch wide, and inserted near one corner of the lamp, that the reflected pencil may not be obstructed in its passage; and a feeder

is inserted in the opposite corner, that the lamp may, if necessary, be supplied without removal; it will, however, contain more than enough for twelve hours' consumption. The plane of the wick is inclined at an angle of 4° or 5° to the axis of the pencil of light, which passes through a slit 0·25 inch long, and 0·01 inch wide in the side of a copper chimney: the chimney is supported on glass feet, to prevent the heating of the camphine by conduction, and for the same purpose the burner passes through a piece of wood. As the burner may occasionally require a vertical adjustment, it passes through a small collar with a set-screw; and as a minute quantity of resinous matter, resulting from the imperfect combustion of the camphine, will sometimes trickle down and interfere with the mobility of the collar, it is protected by a small concave rim attached to the burner. To prevent the escape of light from beneath the chimney, and also to protect the flame from the influence of slight currents of air, at the same time without excluding a due supply, a ring of metal is placed outside the glass supports, which rises about one-fourth of an inch above the bottom of the chimney. To render the combustion of the camphine more perfect, the draught in the chimney is confined by an obtuse conical diaphragm, having an aperture a little longer and wider than the burner, the exact size and position of which are represented in Plate V. figs. 4, 5, 6. The small obliquity of the wick is for the purpose of accumulating as much light as possible in a narrow pencil.

When the camphine is fresh, the lamp will burn for twelve hours without any deposition of carbon on the wick, or sensible diminution of the intensity of the light. The application of gas has not been attempted, as the apparatus would probably be applied in situations not accessible to the ordinary supplies of gas. For the purpose of actual observation, and especially wherever it may be proposed to obtain, not merely relative, but absolute determinations from the registers, it will be found desirable to detach the fine slit from the lamp, and to fix it by a suitable support to an immoveable stand on which the lamp is placed. It is also desirable that one edge of the slit should be adjustible by a fine screw, that the width of the aperture may be varied at pleasure*.

The second object has been attained by a paper prepared with the bromide of silver, which is well known to be affected by rays distributed over a larger portion of

* Since the paper was read the frequent use of the apparatus has suggested various modifications and improvements, both in its construction and practical application; these will be inserted in the form of notes for the sake of preserving the integrity of the original papers, and at the same time presenting all existing information on the subject.

The lamps now in use have been constructed by Watkins and Hill, having a detached slit with a fine adjustment (see fig. 7, Plate V.): much nicety is requisite in their arrangement, to enable the camphine to burn without smoking, at a point nearly approaching the maximum of illumination.

If the lamp and slit by which the base-line is described be similarly fixed in position, and the angular value of the distance between the base-line and the register at any given time determined, the declinations may be read off from the registers, with a limit of error depending on the length of scale and the definition of the line. —May 1847.

the spectrum than any of the other argentine salts, and is on that account preferable for the influence of artificial light. The sensibility of paper prepared with this salt has been found to be greatly augmented by the addition of a small quantity of iodide of silver (figs. 1 and 2, Plate V.). In fig. 1, the development of the impression has been arrested when distinctly visible on the iodized portions, but invisible on the remainder of the paper: in fig. 2, the difference in the clearness and darkness of the several portions of the line is very well marked. A little isinglass is also added, partly because the presence of a small quantity of organic matter appears to assist the catalytic action that ensues on the development of the impression, and partly for the purpose of retaining the salts dissolved with it on the surface of the paper.

The mode of preparation is thus: to a filtered solution of four grains of isinglass in one fluid ounce of boiling distilled water, add ten grains of bromide of potassium, and two grains of iodide of potassium*: when cold, the solution is evenly laid with a camel's hair brush on highly glazed paper in sufficient quantity to thoroughly wet the surface, but not to run off; the paper is then quickly dried by the fire, to prevent the solution being absorbed by the paper. Paper thus prepared may be kept for a considerable time in a dry place; but it is recommended not to prepare at one time more than enough for a week's consumption.

When about to be used, a piece of the above paper is evenly washed over by a camel's hair brush with a solution of fifty grains of crystallized nitrate of silver in one fluid-ounce of distilled water, by the aid of red or yellow light only, and placed damp in the apparatus, as the sensibility of the paper is much diminished by permitting it to become dry. When removed from the apparatus, the latent impression is developed by washing the paper with a saturated solution of gallic acid in distilled water, to which a very small quantity of strong acetic acid is added, when used; the addition of the acetic acid is found to diminish the darkening of the paper. The piece of paper used for each of these registers is half a sheet of folio post paper, torn lengthwise into two strips: to the quantity of gallic acid necessary to wet the surface of one of these pieces, which is about a teaspoonful, the addition of three drops of acetic acid is found to be sufficient. As soon as the impression is sufficiently developed, all soluble matter is removed by washing the paper two or three times in water; and lastly, the image is fixed by washing with a solution of twelve grains of hyposulphite of soda in one fluid-ounce of distilled water. A little practice in the manipulation is necessary for the success of this or any other photographic process. It may here be usefully remarked, that in all photographic processes, the strictest attention to cleanliness, as regards all instruments employed, is indispensably necessary: separate cloths, brushes and glasses should be used, and each retained for its respective purpose, as the smallest undue admixture of the materials will entirely frustrate the object to be attained.

The paper prepared as above described, is placed round the outside of a cylindrical

* See note, p. 67.

French shade, about 10 inches high and $14\frac{1}{2}$ in circumference (figs. 1 and 5, Plate VI.), the corners being held together by a little gum dissolved in acetic acid. The shade, after having been blacked in the inside, is cemented into a cap 1 inch deep in the rim, and having a brass pin about $1\frac{1}{2}$ inch long and a quarter of an inch thick fixed perpendicularly in its centre. A second shade, a little larger than the former, is then placed over it, leaving an annular space about one-eighth of an inch wide; the two cylinders are retained in a concentric position, by placing a sufficient number of turns of tape or ribbon round the rim of the cap of the inner cylinder, to fill up the intervening space. As the sensibility of the paper has been found to be materially diminished by allowing it to become dry, the tape or ribbon is saturated with water, previously to the outer cylinder being placed over it, and a piece of moist lint is placed between the round ends of the cylinders: the paper is thus kept perfectly damp until its removal, after having been in action twelve hours. The pin in the cap of the inner cylinder rests on a pair of friction rollers, and the cap rests against a horizontal roller, Plate V. fig. 8; the rounded end of the outer cylinder also rests upon a pair of rollers, which are adjustible in the direction of the common axis of the cylinders, Plate VI. fig. 1; the axis may thus be always rendered horizontal. By these means, the cylinders revolve so easily that they have been carried round by the hour-hand of a common watch; as however stoppages occasionally took place, from a deficiency of motive power, a stronger movement, such as now in use, is recommended*. A small collar with a set-screw fits on the pin of the cylinder, into which is inserted a bent wire that engages with a fork at the end of the hour-hand of the time-piece (Plate VI. fig. 3), the hour-hand being a little longer than the minute-hand. The cylinder is thus carried round its axis once in twelve hours, and at the commencement of the observation the carrier is so adjusted that the point of light may fall at the top of the paper. From the preceding description, it will be readily understood that by a combination of the vertical movement of the paper with the horizontal movement of the luminous point, the magnetic curve is traced out. The time-scale, which is measured lengthwise in the photographs, is evidently $\frac{1}{12}$th of the circumference of the inner cylinder to one hour, and the dimensions above mentioned, which allow 0·1 inch very nearly to five minutes, have been considered to define the period of any

* The size of the time-pieces in use is that of an ordinary ship chronometer, and in the construction of these a great improvement has recently suggested itself. The motion of the hour-hand is usually reduced from that of the arbor carrying the minute-hand by two wheels and pinions, the play of which, although necessary for their free action, allows a degree of mobility in the hour-hand rarely less than equivalent to five minutes in time, and consequently, without due caution, an error to this amount may be introduced in the register; moreover, the hour-hand is supported by too short an axis to enable it to overcome a considerable resistance applied to its extremity, without acting as a drag upon the whole train. To obviate these inconveniences, the hour-hand is placed on the arbor of the centre or second wheel, which is driven directly by the barrel, and drives the rest of the train. With this and other necessary modifications of the train, the variation of rate, whether the hour-hand moves freely, or is engaged in carrying the cylinder, will be scarcely sensible. In carrying out these views, the author is bound to acknowledge his obligation to the skill and intelligence of Mr. Eiffe.—May 1847.

given variation with sufficient accuracy. As it is extremely improbable that two or more cylinders should be obtained of precisely similar circumference, it is desirable that a correct time-scale should be constructed for each cylinder employed, which may be readily effected in the following manner. Let a piece of paper, made as damp as the photographic paper usually is when placed on the cylinder, be cut so as to exactly meet round it; when dry, let this be divided into twelve equal parts, each of which will represent 1^h; these again into twelve parts, each of which will correspond to 5^m. By this arrangement an allowance is made for the shrinking of the paper*. The angular deviation is measured from a line on the paper drawn in the direction of its motion, such, for example, as would be described by the point of a fixed pencil resting constantly against it; and at the scale of 0·05 inch to one minute (which may perhaps be taken as the best working scale with the existing apparatus), the edge of the line when the various adjustments have been carefully attended to, is sufficiently well defined (as may be observed by a reference to many of the photographs) to determine its place to half or less than half a division of a scale of $\frac{1}{60}$th of an inch, that is, the position of the magnet may be read to within ten seconds. The plan adopted for determining the position of the base-line on the photographs, has been that of drawing pencil marks across the line at which one end of the paper overlaps the other, previously to its removal from the cylinder. When the register is dried, a line drawn through the two corresponding marks at the two ends has been taken to be the base-line. As however it can never be expected to obtain glass cylinders that have either perfectly cylindrical surfaces or perfect surfaces of revolution, the line of intersection of a plane perpendicular to the axis of the cylinder with the paper, would not probably be a perfectly straight line when the paper is opened out; and if the paper should from any cause contract unequally in drying, this line of intersection, which is the true base-line, would be liable to some further distortion. A comparison of many photographs with the Greenwich observations has led to the belief, that a small portion of error has occasionally arisen from these causes; there is, however, no reason for supposing that it has ever exceeded one minute. The means of obviating these sources of error by a photographic base-line are under consideration, but have not yet been perfected †.

* It is questionable whether the inequalities in length of the different registers, arising from different degrees of moisture and corresponding expansion of the paper, and from other causes, may not be too great to admit of the use of any fixed scale; in this case it will be necessary to divide each base-line into twelve parts by a suitable pair of proportional compasses, or some other contrivance. The errors arising from unequal expansion of different parts of the same piece of paper would, it is believed, be too minute to be recognised. In order, however, to guard against these sources of error, and generally for the purpose of marking any given epoch on the paper, the pencil of light by which the base-line is described is shut off by a sliding piece attached to the cover. After remaining so for a few minutes, it is readmitted for a period of ten seconds, and again shut off for a few minutes more, the time of its readmission being recorded. A small dot in the midst of an interrupted portion of the base-line will serve to determine the given epoch on the register, for an example of which see fig. 8, Plate IX.—May 1847.

† In the combined register of the declinometer and bifilar magnetometer, the base-line is now described by a

The following arrangements have been adopted to fulfil the third indication, that of magnifying sufficiently the movements of the magnet. A concave mirror of ten inches focus, and three inches aperture, is attached to a brass stem intervening between the frame which supports the magnet and the suspension chord. In order to reduce as much as possible the influence of torsion on the position of the magnet, the suspension skein consists of six equal bundles of untwisted silk fibres about six feet long, being portions of the same skein of flos silk; these, after being boiled in a solution of bichloride of mercury to render them less hygrometric, were stretched by six equal weights, the sum of which was very nearly that which the whole would subsequently have to support, and after having been left for three or four days to find their position of rest, were firmly bound together: by these means the probable equality of tension and freedom from torsion in the entire chord will, it is believed, be considerably augmented. The mirror may be moved round with the stem through any angle of azimuth, that the reflected pencil of light may proceed in a convenient direction; and the lamp must be placed at such a distance from the mirror that the conjugate focus may be at any required distance: and on this distance depends the amplitude of the scale, on which the variations of the magnet are represented. If the image of the slit is formed at a distance of 7 feet 2 inches from the line of suspension, each minute of angular motion of the magnet will be represented by a change of position of the line on the paper, amounting to $\frac{1}{20}$th of an inch; if formed at 9 feet $6\frac{1}{2}$ inches, $1'$ will be equivalent to $\frac{1}{15}$th of an inch; if at 11 feet $11\frac{1}{4}$ inches, to $\frac{1}{12}$th of an inch; and if at 14 feet 4 inches, $\frac{1}{10}$th of an inch will represent $1'$. The support of the lamp by which its adjustments are effected will be readily understood by an inspection of Plate I. fig. 1*.

The pencil of light forming the image of the slit is intercepted by a cylindrical lens, the axis of which is horizontal, and perpendicular to the vertical plane passing through the image and the centre of the mirror; the lens being placed at its focal distance from the image. By this arrangement, that portion of the pencil which passes through the lens is condensed vertically into a narrow space, without at all interfering with its horizontal movement. The most suitable focal length of the cylindrical lens depends upon the length of range: at the shortest distance above-mentioned, a focal length of 1·5 inch will be found to answer very well; at the next, two inches; and at the longest, three inches.

As the amount of spherical aberration increases considerably as the eccentricity of the reflected pencil is increased, it is desirable that this eccentricity should be the least possible; with this view, the lamp must be so adjusted in the horizontal

separate lamp placed about 9 or 10 inches from the cylinder; a small pencil, the axis of which is perpendicular to the axis of the cylinder, is received by a lens placed at less than its focal distance from the paper, a small portion of the pencil thus condensed is transmitted through a narrow vertical fixed slit, and marks the paper (see fig. 8, Plate IX.). In the combined register of the balanced magnetometer and the barometer, the base-line is described by the barometer lamp, as mentioned in the Supplement.—May 1847.

* The lamp now stands on a fixed pillar to which the fine slit is attached.—May 1847.

plane, that the reflected pencil may pass close by the side of the chimney. It is obvious that the angle contained between the incident and reflected pencils may be diminished to any required extent by increasing the focal length of the mirror, as the lamp will then be placed at a greater distance from it, and by the same means the portion of light incident on the mirror is diminished, but the illumination of the image remains constant, its area alone being altered by varying the focal length of the mirror. Mirrors of 5, 6, $8\frac{1}{2}$, 10, 12, and 15 inches focal length have been tried; and in conjunction with the cylindrical lenses which have been used, the mirrors of $8\frac{1}{2}$ and 10 inches focal length and 3 inches aperture, appear to have produced the best effects. It may be here remarked, that the lenses used have been either cylinders, or portions of cylinders cemented together, filled with water; but with solid glass lenses, and more particularly with achromatic combinations, could such be obtained, it is probable that a better effect would be produced with mirrors of from 12 to 16 inches focus, and $3\frac{1}{2}$ or 4 inches aperture*.

In the adjustment of the lamp, it is necessary that the plane of incidence and reflexion should be perpendicular to a vertical plane passing through the slit and the centre of the mirror. This adjustment may be most conveniently effected by placing a piece of wire horizontally across the slit and the reflected pencil, and having fixed the lamp at such an azimuth that the brightest portion of the pencil may be incident on the centre of the mirror, by moving the lamp vertically until the wire appears to coincide with its image at the centre of the mirror. By this adjustment, the image of the slit, being a portion of a caustic surface, is condensed in the direction of a tangent plane, and the confusion of the rays forming the luminous point is a minimum: the effect may be observed by comparing the line, fig. 3, Plate VII., in which this adjustment was intentionally disregarded, with fig 4, in which it was attended to, all other circumstances remaining precisely the same. It may be here observed, that the line being formed by a portion of a caustic surface condensed, the distinctness of its edge is independent of the distance at which the image is formed, and consequently of the

* The register, fig. 8, Plate IX., has been obtained by means of a mirror of 5 inches aperture, and 20 inches focal length; the reflected pencil being refracted through a combination of two plano-convex cylindrical lenses, one of which has a radius of 2 inches, and the other of 1 inch, the aperture of each being about 60 degrees, the interval between them about $1\frac{1}{2}$ inch, and the more convex nearer to the paper. These lenses have been very beautifully executed by M. Lerebours. The mirror has been by successive trials carefully rendered elliptical, the conjugate foci being at the distances of about 2 feet, and 12 feet 4 inches from its surface. The latter distance was selected as suiting the locality of the Royal Observatory for which the apparatus was constructed. It may not be irrelevant to mention here the means by which the elliptical figure of the mirror was obtained. After being brought nearly to the proper figure, a screen was placed in front of it consisting of narrow concentric annuli of card, moveable on a wire, so that either of them could be placed perpendicular to the surface of the mirror, while the rest remained parallel to it, the lamp being placed at the distance of the nearer focus. By means of this screen the incident pencil might be received upon any annular portion of the mirror separately, and after many successive trials, the figure of the mirror was altered until an image was found to be formed by each annulus at the required distance. It is obvious that on the perfection of the figure of the mirror, the sharpness of the edge of the line will in a great measure depend.—May 1847.

amplitude of the scale. This may be observed by a comparison of figs. 1 to 20, Plate VII., in which the scale is 0·05 inch to 1′, with fig. 5, in which it is 0·1 inch to 1′: the only difference is in the darkness of the line, depending on the intensity of the point of light by which it is described.

In order to prevent the paper being darkened by the influence of stray light, a rectangular cover is placed over the cylinders, with a slit in the side, in the same horizontal plane as the axis of the cylinders, the slit being just wide enough to allow the point of light to pass through it: the paper is thus protected from the rays dispersed by the water lens. The time-piece, cylinders and lens, are placed on a tripod stand with the usual vertical adjustment (see fig. 1, Plate VI.), by which the whole apparatus may be so placed that the lens may receive the brightest part of the image. The surface of the stand, and all parts of the apparatus from which light could be reflected on to the paper are blackened over, and the whole is covered by a second case; in the side of which, towards the reflected pencil, is an aperture guarded by a tube about one foot long, and sufficiently large to admit the reflected pencil in any position that it may assume. The box in which the magnet is inclosed, to protect it from being disturbed by currents of air, and all other objects visible through the tube by an eye placed at the paper, except the mirror, are also rendered black; and so complete is this protection, that not the slightest difference can be perceived in the paper, whether bright daylight is freely admitted through three large windows, or wholly excluded. This apparatus has hitherto been applied to register the variations of the declination magnet only, but it may be considered equally applicable, with appropriate modifications, to record the variations of the horizontal and vertical elements of magnetic force.

The experiments have been conducted under the unfavourable influence of that constant tremor which exists in a London thoroughfare: this has been as far as possible counteracted by attaching to the magnet, about half-way between the point of suspension and its extremity, a piece of copper wire, the end of which dips into a glass vessel of oil; this does not appear to have interfered with the movements of the magnet*. The effect of the damper, in arresting the influence of local causes on the vibrations of the magnet, may be best appreciated by a reference to fig. 19, Plate VII., a register taken during a *maximum* local disturbance, namely, a quadrille party in the next house, from the party-wall of which the magnet has been suspended. The unsteady movement of the magnet during the period of the dance (the termination of which is very well-marked) is strikingly contrasted with those periodically augmented and diminished vibrations of the magnet about its mean place, the existence

* The use of oil as a damper is liable to the objection that it may congeal at low temperatures, and thus impede the movement of the magnet; and on this account a slip of wood attached to the magnet, and dipping into a glass vessel of mercury, has been substituted. The use of mercury seems moreover desirable on account of its gravity when so large a concave surface as that of a mirror of 5 inches aperture is exposed to the influence of currents of air, which would in all probability produce more or less vibration in the magnet.—May 1847.

of which has frequently been noticed, and which are very conspicuous in fig. 20, a portion of a register purposely taken during the most quiescent period of the week.

Notwithstanding the local sources of error above alluded to, it is very satisfactory to observe the close agreement between the photographs and several hundreds of the observations made at the Royal Observatory, which have been very carefully laid down upon them. These automatic registers include the two last term-days, and several periods of unusual disturbance, during which extraordinary observations have been made at Greenwich at intervals of one, two, or three minutes, and throughout the whole series the number of discrepancies is very small. An almost continuous registration has been maintained during the present year; and it is due to the praiseworthy vigilance of the observers engaged at the Royal Observatory to state, that very few extraordinary disturbances have escaped their notice.

Of those which have been compared with the Greenwich observations, fig. 20, Plate VII., the register of April 6th, 1846, and fig. 1, Plate VIII., that of April 16th, exhibit the greatest disturbance; in both instances it may be remarked that the greatest variation has occurred between 7^h and 11^h. It may here be conveniently remarked, that in accordance with the practice of the Royal Observatory, the time adopted is Göttingen mean astronomical time, which is $39^m\ 46^s$ in advance of Greenwich mean time.

Fig. 17, Plate VII., the register of April 15th, exhibits a brief disturbance, occurring between 8^h and 9^h, in which the photograph differs more essentially from the Greenwich observations than in any other instance: a sharp cusp in the latter, corresponding at two points to a mere bend in the former, would lead to the inference, that the disturbing cause must have been so near to Greenwich as to have exerted a materially greater influence on the magnet there under observation.

A singular fact is established by some of these photographs, namely, that after a certain space of time the actinic influence spontaneously decays; for in some instances the paper has been suffered to remain on the cylinder more than twelve hours; and consequently a second impression is made upon the paper within a very small distance of that which had been made twelve hours previously, and while the latter impression is distinctly developed, the former is very faint: fig. 6, Plate VII. is a good illustration of this fact. The succeeding portions however of the first impression become more and more distinct, thus showing that the decay is gradual. It appears to commence after a period of from ten to fourteen or fifteen hours; but the conditions on which this variation of time depends have not been ascertained*.

* Subsequent observation has led to the opinion that this decay of the impression depends on the paper having been originally prepared with too weak a solution of bromide of potassium. The solution now used contains 4 grains of isinglass, 16 grains of bromide of potassium, and 4 grains of iodide of potassium in one fluid-ounce of distilled water. The paper prepared with this solution appears to retain the actinic impression unimpaired for a period of more than 24 hours. Consequently two registers, each of 12 hours' variation, may be obtained on the same paper by allowing the cylinder to go twice round. An example of this may be seen in Plate IX. fig. 8, in which it may be observed that the occasional crossing of the lines does not at all interfere with their

The most violent shock that has been recorded, fig. 10, Plate VII., occurred between 5^h and 6^h on the 1st of April. On this occasion, the magnet, after suffering two small shocks, was at 5^h 20^m suddenly displaced as by a blow, and thrown into a wide oscillation, from which it did not return to a state of rest for 25^m; it then sustained a considerable shock in the opposite direction, from which it again returned to a state of rest, in nearly its original position, about 6^h.

It is confidently anticipated that a continuance of these observations during the approaching period of the year, when disturbances are usually most frequent and considerable, may lead to some interesting results.

distinctness. By this means half the trouble of changing the papers and developing the impressions is saved, and the relative positions at intervals of 12 hours may be determined with a much greater degree of certainty, the only source of error being in reading the position of the edge of the lines on a scale.—May 1847.

29 *Keppel Street,*
June 16, 1846.

VIII. *On the Automatic Registration of Magnetometers, and other Meteorological Instruments, by Photography.* By Charles Brooke, *M.B., F.R.C.S.E.* *Supplement.* *Communicated by* G. B. Airy, *Esq., F.R.S., Astronomer Royal.*

Received November 23,—Read November 26, 1846.

During the period of the summer recess, the system of automatic meteorological registration by photography has been rendered complete by the adaptation of the barometer and thermometer to the apparatus previously described. It having been found a matter of much difficulty to obtain a photographic base-line from the lamp already described as being placed near the magnet, the idea naturally arose that the base-line might be simultaneously described by a second lamp placed on the opposite side of the cylinder, as represented in fig. 1, Plate V. A pencil of light proceeding from this lamp through a horizontal slit in the chimney is received by a cylindrical lens placed, as before, horizontally, and the focal line of light thus formed is allowed to pass through a corresponding slit in the covering of the cylinder. A small section only of this focal line is transmitted through a vertical slit in a piece of thin sheet brass attached to the stand on which the cylinders rest, and placed very near the surface of the outer cylinder. A line thus described may be seen in Plate VIII. fig. 4, and Plate IX. figs. 6, 7, 8, and the same light has been by the following means rendered available for the registration of the barometer. A siphon barometer has been constructed with a column of mercury a little more than one inch in diameter, Plate VI. figs. 1 and 2. As the weight of an entire column of this size would be inconvenient, and as it would be difficult to obtain a tube more than three feet long of so large a bore, both ends of which were of the same internal area, two adjacent short pieces of a very nearly cylindrical tube have been united to the extremities of a tube of small bore, and form the ends of the instrument which contain the surfaces of the mercury. A wooden cap about two inches high is fitted to the open end of the tube, at each end of which are fixed three small friction rollers, placed radially, vertical, and equidistant from each other. The stem of a glass float, having a bulb about half an inch in diameter, resting on the surface of the mercury, passes up vertically between these friction rollers, by which the free vertical movement of the float is much facilitated. At the upper end of the stem is a cap containing a small grooved roller. The barometer tube is attached to a board by two clamps, so as to be capable of being raised or lowered at pleasure, and the bend at the lowest part rests on a piece of wood, which is likewise capable of a vertical adjustment. Another piece of wood, about half an inch thick, two inches wide, and five or six long, is made to slide horizontally between

two slips fixed to the surface of the board at such a height that the top of the float may be opposite the middle point between them. To this sliding piece a pulley about three inches in diameter, having a fixed axis about 3 inches long, is attached by a suitable support; to this pulley two slender wooden arms are attached, one thirty inches, the other five inches long, and fixed at right angles to each other*. A piece of wire with an adjustible balancing weight is fixed in the pulley in such a position that the axis of the pulley may be the centre of gravity of its appendages. The long arm passes through a slit in the stand of the apparatus, and carries a black paper screen with a vertical slit in front of the horizontal aperture in the cover above described (see Plate V. fig. 2); and is so placed that the point at which the slits cross each other is exactly thirty inches from the axis of the pulley. The short arm rests on the roller at the end of the float, and is marked at the distances of 3, 3·75, and 5 inches from the axis of the pulley. The mark which rests on the float may be changed at pleasure by sliding horizontally the piece to which the pulley is attached; and accordingly as the marks are respectively placed in the above position, it is evident that the movement of the point of light transmitted through the slit in the moveable screen will be five, four, or three times the variation in the height of the column of mercury; and thus by the same lamp the base-line and the barometric curve are traced out. Of this, fig. 4, Plate VIII. and fig. 7, Plate IX. are given as examples. In these it may be remarked that both the lines are so sharply defined, that by applying a scale divided into $\frac{1}{100}$ths of an inch, the position of both may be read to half a division, which is equivalent to 0·001 inch of mercury, if the first scale be adopted, which has been the case in these instances.

A small weight suspended by a string passing round a groove in the pulley keeps the short arm in contact with the float, by a constant pressure. There being an annulus of mercury rather more than one-fourth of an inch wide between the tube and the float, the effect of capillarity is so much reduced as to exert scarcely any influence on the variations of the column, the weight of which is sufficient to overcome the small amount of friction that exists in the various parts, without sensibly influencing its variation, and consequently the barometric curve is frequently continuous, and not interrupted by jerks. In one of the registers, not introduced for want of space, the passage of an aërial wave is recorded, equivalent to less than $\frac{1}{300}$th of an inch of mercury, the duration of which was about $4\frac{1}{2}$ minutes.

The lamp being placed at a distance of about nine inches from the paper, the direction of the small pencil by which the curve is traced varies considerably; hence an error is introduced in the register, equal to the distance of the slit from the paper multiplied by the cotangent of the angle at which the ray is inclined to it; this however may be either allowed for, or obviated by rendering all the rays of the pencil parallel to the same vertical plane by means of a cylindrical lens, placed at its focal distance from the lamp.

* The pulley and slide have since been made of brass.—May 1847.

A continuous registration of the variations of the thermometer has been obtained by intercepting the focal line of light formed on the paper as above described, by the stem of a thermometer having a wide flat bore. A sufficient quantity of light passes through the empty portion of the bore to darken the paper, but is entirely excluded from the portion occupied by the mercury. The register therefore consists of a light and a dark space, separated by a well-defined boundary line, the distance of which from the base-line will furnish the required indication. This particular application of the apparatus prefers no claim to novelty, as a very similar means of registering the variations of the thermometer has already been published*, and is here introduced merely as forming a necessary part of a complete system of automatic meteorological registration.

As a thermometer with a large bore, and a scale sufficiently open to give the indications of change with the requisite degree of minuteness, must of necessity contain a large quantity of mercury, which if contained in a globular bulb would not be sufficiently sensitive, the instrument which has been used has a long narrow tubular bulb (see Plate VI. fig. 11), by lengthening which any required amplitude of scale may be obtained without any diminution of sensibility.

As small differences of temperature have a much greater influence at low temperatures in determining the hygrometric conditions of the atmosphere, from a comparison of the thermometer and psychrometer, than they have at higher temperatures, and as the range of variation is so much less during winter than in summer, it is proposed that a thermometer and psychrometer, having scales of about five degrees to one inch, should be used in winter; while those for summer use should have a scale of about ten degrees to one inch.

It having been found practically impossible to depend on the uniformity of a wide flat bore in a glass tube, a more than usually correct method of graduating these instruments would be desirable. This object would be attained by fixing to the stem a scale of $\frac{1}{20}$ths of an inch, which by a suitable vernier might be read with any required degree of minuteness. A separate comparison of the readings of this scale with two or more good standard thermometers should now be made, each being immersed with the instrument to be graduated in a vessel of warm water, which is allowed to cool very slowly. A mean of the results thus obtained would probably afford a very nearly correct graduation, which would, for the reasons above stated, be of most importance at low temperatures.

If the register is required to furnish only differential results, a great length of stem may be obviated, and the safety of the instrument secured, if casually exposed to a temperature above its range, by a safety bulb at the upper end of the stem, as represented in the diagram. By retaining a certain quantity of mercury in this bulb, the mean temperature corresponding to the time of year may be always made to occupy nearly the same place, and a change of thermometers thus rendered unnecessary.

* See Engineers' Magazine, Nov. 1845.

The six meteorological instruments of which a continuous registration is proposed to be obtained by the means above described, are the declination magnet, the horizontal and vertical force magnetometers, the barometer, the thermometer, and the psychrometer; and by the arrangements above described, two of these may be registered on the opposite sides of each of three cylinders; the declination and horizontal force magnetometers on a cylinder placed horizontally, as represented in the Plate. In this case the describing a base-line would be most readily effected by a third lamp; The horizontal force magnetometer and the barometer on a vertical cylinder, the long arm of the barometer index being placed horizontally; and the thermometer and psychrometer on a second vertical cylinder, which must necessarily be placed in the open air. This arrangement will not however be attended with any difficulty, if the lamps are inclosed in a case similar to a magic lanthorn, the chimney of which is protected from the influence of descending currents, either by a revolving cap with a spiral lamina attached to it, or by FARADAY's ingenious expedient of interrupting the continuity of the chimney at two or more points, by parallel conical surfaces. The apparatus for carrying the vertical cylinders is represented in Plate VI. figs. 4 to 8.

Figs. 9 and 10, Plate VI., are views of a carrier for the bifilar magnetometer. The stem to which the mirror is attached has a stirrup to hold the bar at the lower, and a torsion circle at the upper end. To the index-plate which moves with stiff friction on the torsion circle is attached a right- and left-handed screw, carrying two pulleys under which the suspension skein passes. These may be adjusted by the screw to give the requisite degree of sensibility to the magnet. A more accurate adjustment of the angle of torsion may, if requisite, be obtained by a tangent screw attached to the index, and gearing with the circumference of the torsion circle.

Keppel Street,
November 23rd, 1846.

Postscript.—In the endeavour to avoid prolixity, the author may perhaps have omitted some details which would have facilitated the construction of the apparatus. On some few points, on which circumstances have appeared to render further information necessary, that has been embodied in the description of the Plates: he will, however, be at all times ready to further the objects of science, by communicating any required details to those who may be disposed to make a practical application of them.

The author takes this opportunity of publicly and gratefully expressing his thanks to the Council of the Royal Society for their liberal contribution towards defraying the expenses of the apparatus.—*May* 24, 1847.

Fig. 8.

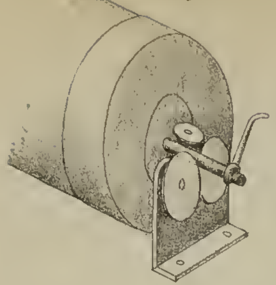


Fig. 7.

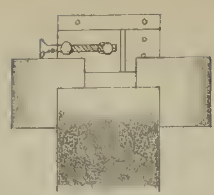


Fig. 1.

Fig. 2.

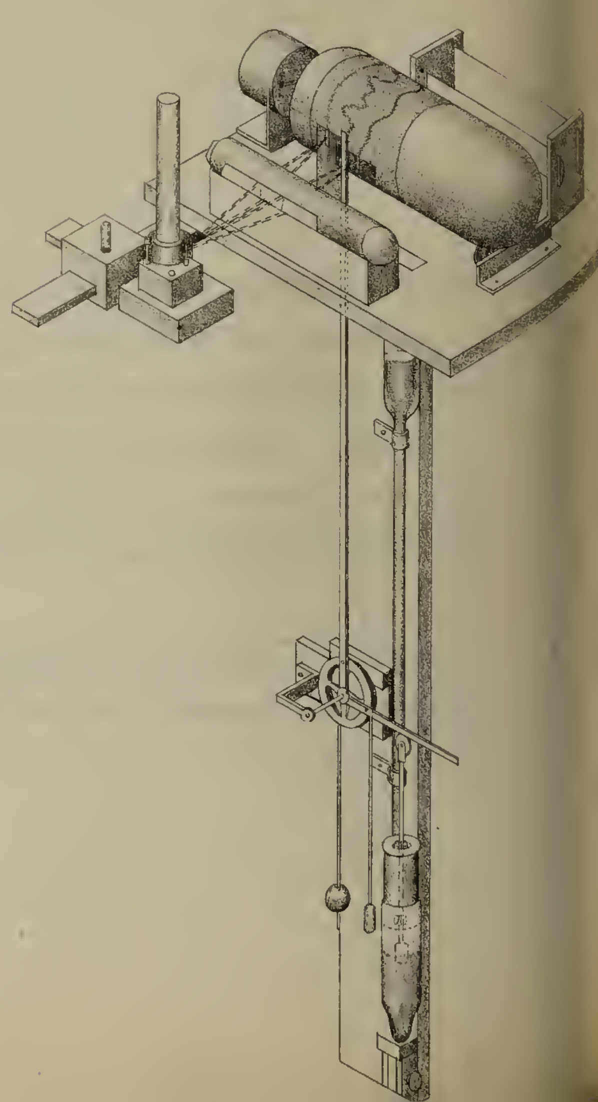


Fig. 4.

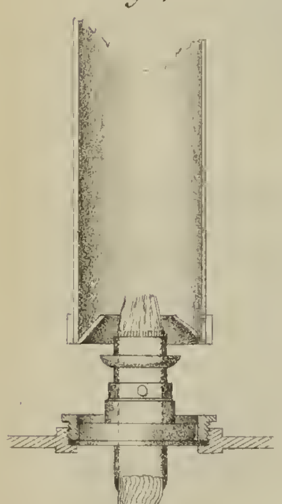


Fig. 3.

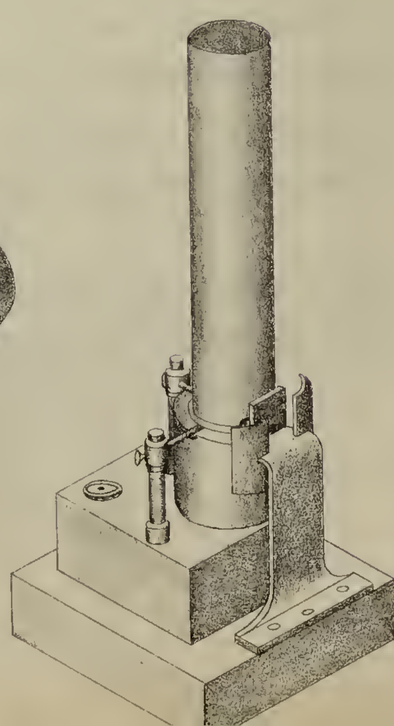


Fig. 5.

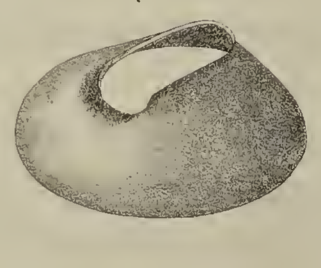
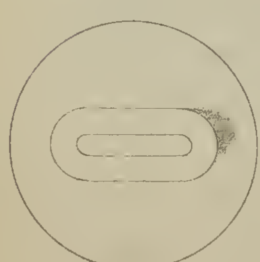


Fig. 6.



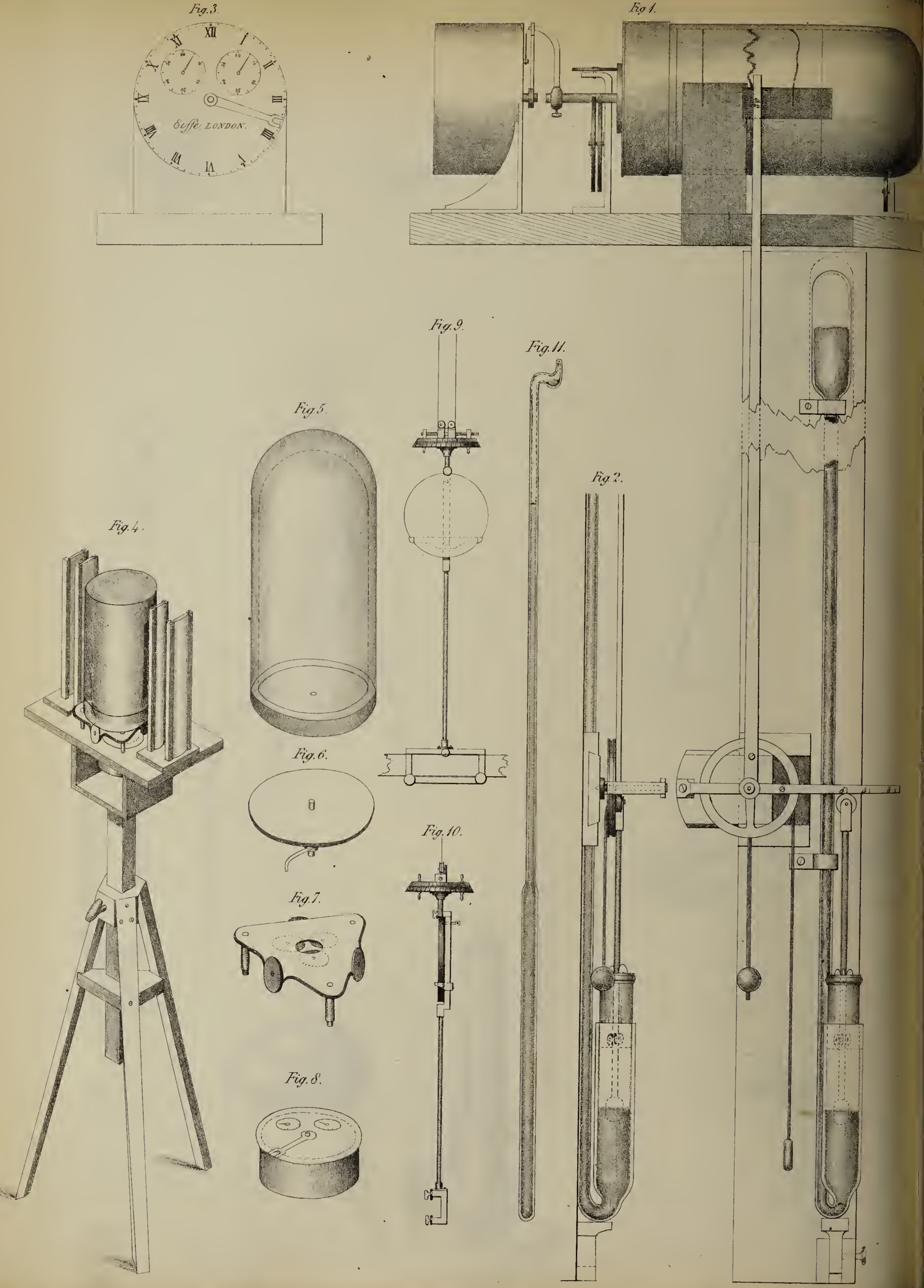


Fig. 3.
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Fig. 1.
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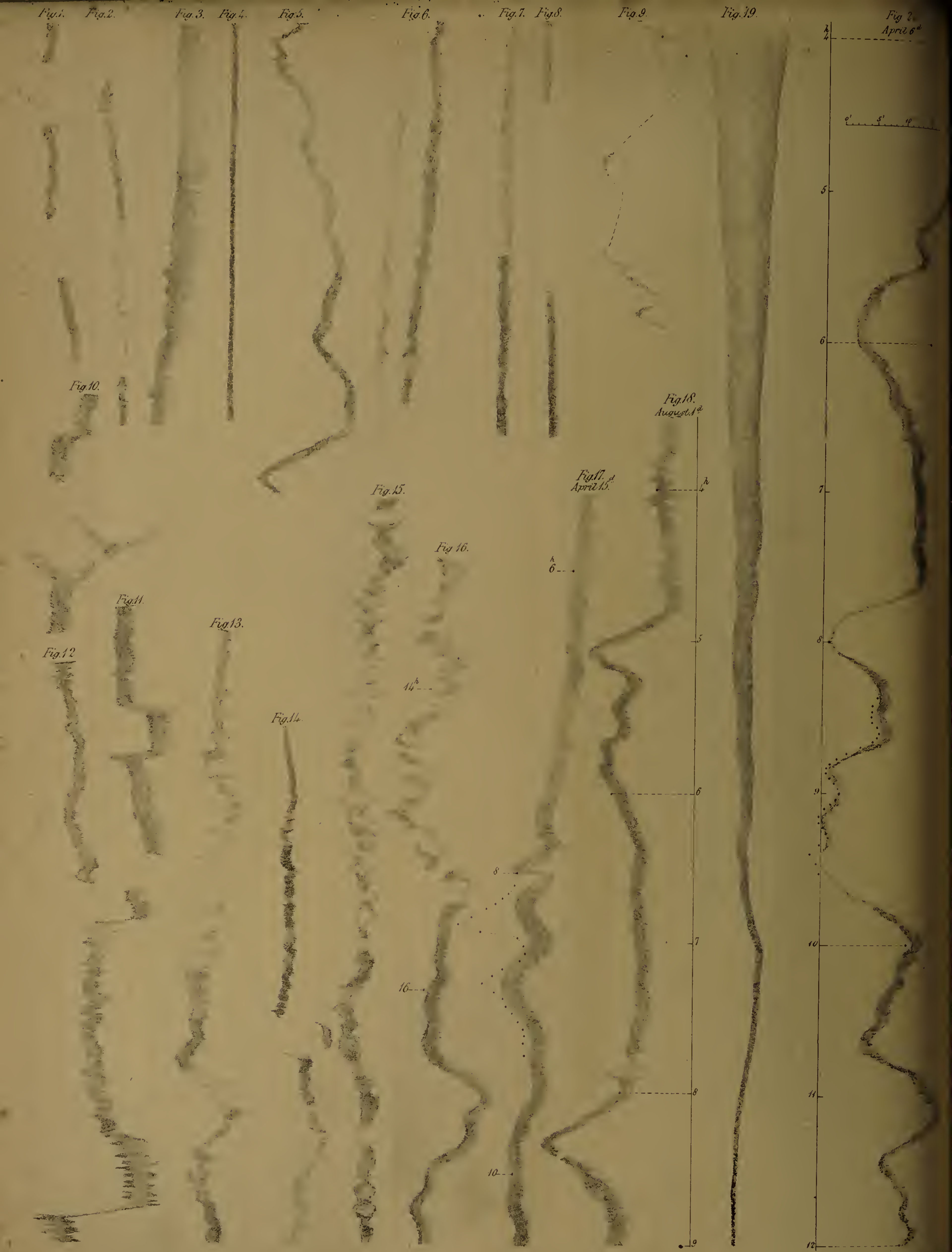
Fig. 10.
Fig. 7.
Fig. 8.



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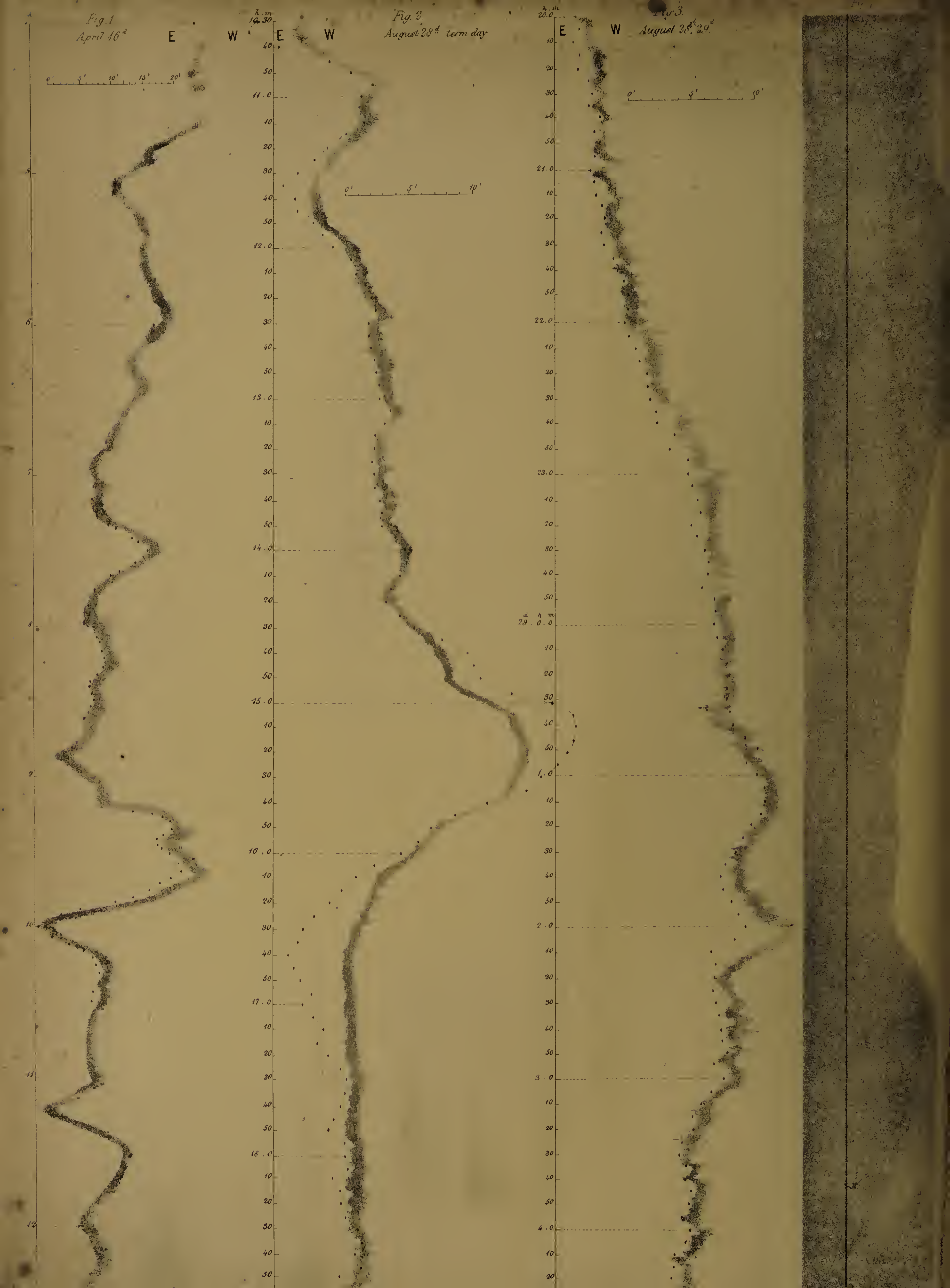
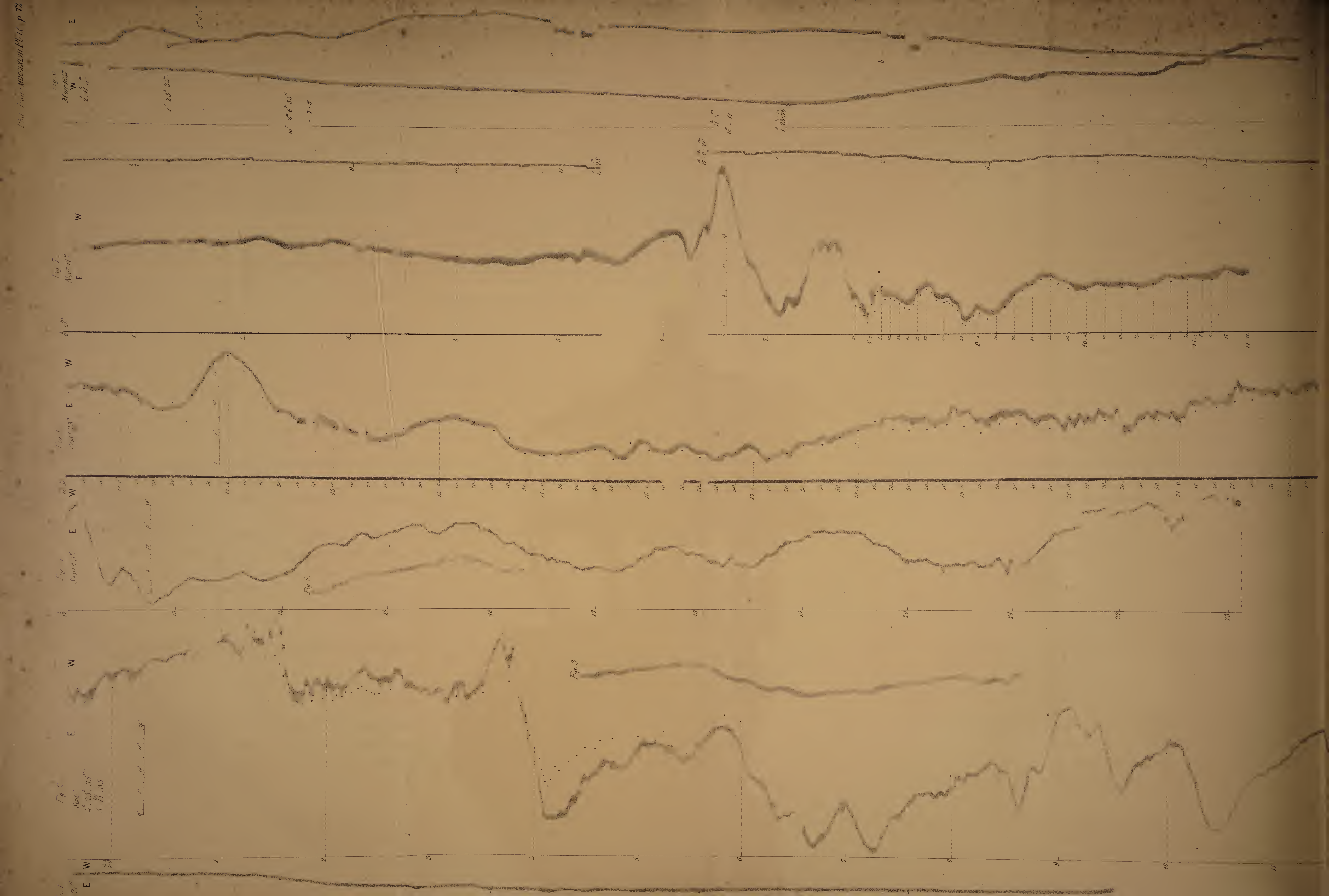


Fig. 1
April 16d
E W
0' 5' 10' 15' 20'
Fig. 2.
August 28d term day
E W
0' 5' 10'
Fig. 3
August 28d 29d
E W
0' 5' 10'



Description of the Plates.

PLATE V.

Fig. 1. A sketch of the complete apparatus, as used in combining the registers of the declinometer and barometer. The combination of two plano-cylindrical glass lenses, as used in obtaining the register fig. 8, Plate IX., is here represented.

Fig. 2. A sketch of the barometer side of the apparatus, with lamp, &c. The cylindrical water lens, as originally used, is represented in this figure.

Fig. 3. A sketch of the lamp in its stand, to which the fine slit is attached.

Fig. 4. A section of the burner, the wooden socket, through which it passes, the chimney and the diaphragm; one-half the real size. The top of the burner should stand from 0·22 to 0·25 inch below the aperture in the diaphragm, and the wick should rise from 0·04 to 0·05 inch above the top of the burner. The illuminating power, and steady burning of the lamp without smoking, entirely depend on the correct adjustment of these distances, if the camphine is not oxidized by too long keeping.

Fig. 5. A sketch of the diaphragm, full size, showing a notch or depression of a portion of the edge intervening between the flame and the slit, for the purpose of allowing the passage of rays from all the most luminous portion of the flame, some part of which is below the level of the edge of the diaphragm.

Fig. 6. A plan and elevation of the diaphragm, full size; to show the size and position of the aperture.

Fig. 7. An elevation of the adjustible slit. As this is in contact with the chimney, its support necessarily becomes very hot; and in order to diminish the transmission of heat to the lamp, a space of about 0·1 inch is left between the support and the ring of metal described as surrounding the burner; the wings represented in this figure intercept the light that passes through the space here mentioned.

Fig. 8. The friction wheels which support the axis of the horizontal cylinders.

PLATE VI.

Fig. 1. A side view of the cylinders and time-piece, showing their connexion, and an elevation of the barometer, showing the several parts described. If the registers of the balanced magnetometer and barometer are combined as proposed, the relative positions of the index, its slit, and the cylinders will remain the same.

Fig. 2. A side view of the barometer.

Fig. 3. An elevation of the dial and hands of the time-piece, showing the fork at the

extremity of the hour-hand, that engages with the carrier on the axis of the cylinder.

Fig. 4. A sketch of the apparatus for carrying a vertical cylinder, with the requisite arrangement of lenses for combining the registers of the balanced magnetometer and barometer, or of the thermometer and psychrometer.

Fig. 5. A vertical cylinder, with its outer cylinder. A hole in the centre of the cap fits over a pin in the centre of the plate on which it rests, and is carried round.

Fig. 6. The horizontal plate on which the cylinder rests. The under surface of this plate rests on the edges of three equidistant vertical friction wheels or rollers, attached to the frame beneath it. A vertical pin fixed to the centre of the under surface of the plate passes down between the edges of three equidistant horizontal rollers, which are attached to the under surface of the frame, and on this pin the carrier is fixed.

Fig. 7. A frame supported on three legs, to which the friction rollers above described are attached. By this arrangement the motion is so easy that the effect of the drag on the rate of the time-piece is scarcely sensible.

Fig. 8. The time-piece, the rim of which is let into the surface of the stand. In doing this great care should be taken that the axis of motion of the hour-hand is continuous with the axis of the cylinders, and of the plate on which they rest. In registering the balanced magnetometer, as in the declinometer, the cylinder makes two revolutions in 24^h, and two 12^h lines are traced on the paper; but for the thermometers, the cylinder, and consequently the hour-hand makes only half a revolution in 24^h, and each of the two thermometers is registered on one-half the paper during that time. The cylinder is about 19 inches in circumference, which suits the length of a sheet of folio post writing-paper.

Fig. 9. An elevation of the bifilar carrier.

Fig. 10. A side view of the same. The centres of gravity of the carrier, the mirror, and the magnet, are very nearly in the same vertical line.

Fig. 11. An elevation of the thermometer tube.

PLATE VII.

Figs. 1, 2. Show the increased sensibility of the paper by the addition of a very small quantity of the iodide of potassium to the first solution.

Fig. 3. Shows the effect produced upon the line by the want of horizontal adjustment of the plane of incidence and reflexion, compared with

Fig. 4, in which this adjustment has been carefully made, all other circumstances remaining the same.

Fig. 5. Specimen of a well-defined line, on the scale of 10′ to 1 inch.

Fig. 6. The development of a recent impression, and the decay of another made twelve hours previously, and very near the former.

Figs. 7, 8. The comparative effects of the camphine and oil lamps, they having been exchanged for the purpose of experiment.

Fig. 9. The commencement of the magnetic storm of September 5th, 1846. The oil lamp was in use, and this shows its inability to impress the photographic paper during rapid movements of the magnet, when the registration is most important.

Fig. 10. The greatest magnetic shock that has been observed during the year*. It occurred on April $1^d\ 5^h\ 20^m$.

Figs. 11, 12, 14. Examples of shocks, of these No. 12 is the least impulsive. It may be remarked, that in these and in all other recorded shocks, the first impulse has been to the west, and has been followed shortly after by a second to the east.

Fig. 13. Tremor continuing for more than an hour. This occurred on April 25^d, between 20^h and 22^h, and from the habitual quietude of the streets at such a time, cannot probably be attributed to local causes.

Fig. 15. A register of July $12^d\ 3^h$ to 8^h, showing an unusually frequent repetition of small oscillations. These oscillations, which have been frequently noticed, differ considerably from the shocks; the magnet appears to be disturbed from a state of rest by gradually increasing oscillations, which, after having attained a maximum, subside in nearly an equal time: the period of these disturbances is from two or three to ten or fifteen minutes.

Fig. 16. The effect of a maximum local disturbance—a quadrille party in the adjoining house. The unsteady movement of the magnet is well-contrasted with the preceding disturbances.

Fig. 17. A disturbance which occurred on April $15^d\ 8^h$ to 9^h. This differs more essentially from the Greenwich observations than any that has been compared: in the curve laid down by points from the observations made at the Royal Observatory, two sharp cusps correspond in time to mere undulations in the register. May it not be inferred that in this instance the disturbing cause was sensibly nearer to Greenwich than to Keppel Street?

Fig. 18. The register of August $1^d\ 2^h$ to 11^h, during which period the great hail-storm occurred.

Fig. 19. A register obtained at the Royal Observatory, without a damper. The oscillations of the magnet gradually diminished, and after $6^h\ 40^m$ it returned to a state of rest.

Fig. 20. The register of April $6^d\ 1^h$ to 13^h, one of the great disturbances of the year 1846. A very close agreement may be observed between the register and the Greenwich extraordinary observations, made between 8^h and 10^h. In all the figures of this Plate, except fig. 5, the scale is that of $20'$ to 1 inch.

* All the dates except that of fig. 8, Plate IX. refer to the year 1846.

PLATE VIII.

Fig. 1. Register of April 15^d 4^h to 12^h, another great disturbance. The Greenwich observations made during the principal part of the disturbance agree well with the register. Scale 20′ to 1 inch.

Figs. 2, 3. The greater part of the term-day, August 28^d 10^h to 29^d 10^h, on the scale of 10′ to 1 inch. These registers show a general agreement with the Greenwich observations; but it appears that the excursions of the registered magnet have been generally less than those of the Greenwich magnet. No. 3 shows a constant succession of small disturbances.

Fig. 4. A register of the thermometer and barometer combined, the two instruments being placed on opposite sides of the cylinder. The base-line drawn by the barometer lamp may be distinguished through the paler tint produced by the light passing through the empty portion of the thermometer tube.

PLATE IX.

Fig. 1. Register of August 20^d, showing an unusual absence of disturbance: this is not however a solitary instance.

Fig. 2. Register of September 5^d 0^h to 11^h 30^m, the greatest magnetic storm of the year 1846. It may be remarked, that the excursions of the registered magnet, though agreeing in direction, were considerably greater in extent than those of the Greenwich magnet. The contrary took place in figs. 2, 3, Plate III. This and the preceding are on the scale of 20′ to 1 inch.

Fig. 3. Portion of a register on another piece of the same sheet of paper as the preceding, but on the scale of 10′ to 1 inch; to show, *cœteris paribus*, the relative intensity of the actinic rays.

Fig. 4. Register of September 5^d 12^h to 23^h, showing the gradual subsidence of the storm. Scale 20′ to 1 inch.

Fig. 5. Portion of the succeeding register of September 6^d 0^h to 11^h, on part of the same sheet as 4. Scale 10′ to 1 inch. The line is equally well-defined, but fainter.

Fig. 6. Register of a term-day, September 23^d 10^h to 22^h, with a photographic base-line; on which the Greenwich observations were laid down at the Royal Observatory. This register well illustrates a fact that has been frequently noticed, namely, the gradual subsidence of slow excursions into small and brief disturbances. The scale is 15′ to 1 inch.

Fig. 7. Another of the great disturbances of 1846; November 17^d 0^h to 12^h with base-line, and register of barometric variations magnified five times. The nearly horizontal portion of the line between 6^h 20^m, and 30^m, indicates the most rapid movement of the magnet that has been recorded by photography. No point of it could have been illuminated more than 16^s. The

scale is 15′ to 1 inch. This register exhibits a very remarkable accordance with the extraordinary observations made at the Royal Observatory during the latter part of the disturbance. Two sharp cusps at $8^h\ 2^m\ 30^s$ and at $8^h\ 25^m$ may be specially noticed. The advantage of automatic registration is here manifest; the magnet having been during several previous bihoral observations very steady, as may be seen from the first part of the line, there was no previous indication of the disturbance which commenced shortly after 6^h: and the attention of the assistant on duty was directed to the magnets only by his casually observing that the balanced magnetometer had undergone a very remarkable deflection. The sudden breaks in the barometer line are due to a small amount of friction that has since been obviated.

Fig. 8. A register of the declinometer from May 1847, $1^d\ 23^h\ 34^m$ to $3^d\ 0^h\ 4^m$, made in Keppel Street by the apparatus constructed for the Royal Observatory. There is no perceptible difference in intensity between the commencement and termination of the line. Several shocks may be remarked, the W. declination having been, as usual, increased by the disturbing cause, which appears to have commenced and terminated abruptly. In some instances the duration of very transient disturbances may be reasonably conjectured from the tint of the marks on the paper, those for instance at *a*, *b* have not probably exceeded 20^s. The absence of vibrations, as in the shocks represented in Plate VII., is probably owing to the inertia of the mercury damper, and offers an additional argument in favour of its use. The base-line is drawn by a lamp on the opposite side of the cylinder, and shows two of the 10^s marks previously described. The narrowness and sharpness of the base-line will materially assist in diminishing the probable error of reading the position of the register by a scale.

IX. *On the Proper Motion of the Solar System.*
By Thomas Galloway, *Esq., M.A., F.R.S., Sec. R.A.S.*

Received March 4,—Read April 15, 1847.

THE third volume of the Mémoires presentés par divers Savans of the Imperial Academy of St. Petersburg, published in 1837, contains a paper by Professor Argelander, in which that distinguished astronomer has discussed the question of the proper motion of the solar system, and determined the probable situation in space of the point towards which the sun is at present advancing. This determination was founded on the proper motions of 390 stars situated between the north pole and the tropic of Capricorn, as shown by a comparison of their positions in 1775 according to Bradley's observations, reduced by Bessel, with their positions in 1830 computed from the observations made by Argelander himself at Abo; every star being taken into account which appeared to have a proper motion amounting to a tenth of a second in space annually. Two other investigations of the same question have since been published; one by Lundahl, founded on the proper motions of 147 stars, as shown by a comparison of the observations of Bradley and Pond, and the other by Otto Struve, based on 392 stars, whose proper motions were determined by a comparison of Bradley's observations with those made at the observatory of Dorpat. From these three investigations the direction of the sun's motion in space may be considered, perhaps, to have been determined with as great an approximation to accuracy as can be attained in the present state of our knowledge of the proper motions of the stars in the northern hemisphere. The recent catalogues of Mr. Johnson and the late Professor Henderson, deduced from the observations made by those astronomers respectively at St. Helena and the Cape of Good Hope, on being compared with the Cape observations of Lacaille made about the middle of the last century, show that a considerable number of the southern stars have also very appreciable proper motions; and it appeared to me to be a matter of some interest to inquire whether the proper motions so determined afford any confirmation of the results obtained by Argelander, Lundahl and Struve, or favoured the hypothesis of a displacement of the solar system. The result of this inquiry I have now the honour of submitting to the Royal Society, in whose Transactions the existence of relative displacements among the fixed stars was first announced, and the probable direction of the sun's motion first indicated. Independently of theoretical considerations, the subject is of much importance in astronomy. The proper motions of the stars, which may be said to be the only residual astronomical phenomena now remaining to be

accounted for by theory, mix themselves up with the determination of the precession and other fundamental elements; and the first step towards acquiring any knowledge of their laws, quantities, or directions, is obviously to distinguish between what is real and what is only apparent, and to separate from the whole observed displacement the effect due to the motion of our own system.

Before proceeding to describe the data and results of the present investigation, it will be desirable, perhaps, to give a brief notice of the principal inquiries that have heretofore been undertaken with reference to the same subject.

In the Philosophical Transactions for 1713, Halley first called attention to the circumstance that a comparison of the ancient with modern observations showed that three of the principal stars, Sirius, α Tauri and Aldebaran, had changed their positions relatively to the fixed circles of the sphere, and advanced considerably towards the south. Until this time the notion had universally prevailed that the places of the stars are subject to no relative change; but no sooner was the notion called in question than instances of such change were multiplied; and the proper motions of stars being once admitted, it was naturally suggested that the sun itself partakes of a similar motion. Bradley, in the memorable paper in which he announced the discovery of the nutation, published in the Philosophical Transactions for 1748, described the appearances which would result from a change of the position of the solar system in absolute space, but he made no attempt to explain the observed phenomena on this hypothesis, and remarked that the alterations in the relative positions of the stars might arise from so great a variety of causes, that many centuries, perhaps, would be required to discover their laws.

Tobias Mayer, in a memoir presented to the Göttingen Society in 1760, and published among his Opera Inedita in 1775, gave a list of eighty stars which had been observed by Rœmer in 1706, and compared their places as given by Rœmer with those deduced from his own observations (1756) and those of Lacaille (1750). Out of the eighty stars about fifteen or twenty were found in respect of which the difference of position, either in right ascension or declination, exceeded 15″, a quantity which he considered would be at least equal to the error of observation. In the cases, therefore, in which the difference did not much exceed 15″, he thought a proper motion was not improbable; but in some cases, as those of Arcturus, Sirius, Procyon, α Aquilæ, Piscis Austrinus, and a few others, the difference was so great that there could be no question about the existence of such motion. Mayer also made the remark, that the stars which appear to change their places are not confined to those of the first or second order of magnitude, which, by reason of their greater brilliancy, might be presumed to be the nearest to the earth; and that among the brighter there are some which appear to be altogether at rest. And he further remarked, that although it is by no means improbable that the sun as well as the stars may have a motion of its own, yet, as the observed changes of position do not follow the law they would observe if caused by the motion of our solar system towards a given point in

the heavens, it is manifest they do not proceed from this or any similar common cause, but belong to the stars themselves, though the true and genuine cause may remain unknown for ages.

This conclusion, if understood as applying to the whole of the changes of position indicated by the comparison of the catalogues, was no doubt correct; but it is evident that, although the apparent displacements may not be capable of being completely explained on the hypothesis of a solar motion, it by no means follows that they do not in part depend upon this cause, and that, widely as the observed motions may differ in their relative directions, there may not still be a preponderance of motion, or a general tendency to move, towards some determinate point. Lambert, writing in 1761, remarked that the apparent changes in the positions of the fixed stars depend on the motion of the sun as well as on the motions of the stars themselves, whence, he says, "we may perhaps in time arrive at the means of determining towards what region of space the sun holds its course." The same philosopher also noticed that the rotation of the sun on its axis gives rise to a probability of its translation in space, although no proof can be given that the latter motion is a necessary consequence of the former.

The probability of the opinion that the observed proper motions of the stars are compounded of a real and an apparent motion was also noticed by Michell, who, in a note to a paper published in the Philosophical Transactions for 1767, remarked that the apparent change of situation which has been observed in a few of the stars, is a strong circumstance in favour of the opinion that those stars are among the nearest to us; and that the apparent displacement may be owing either to a real motion of the stars themselves, or to that of the sun, or partly to the one and partly to the other. And, he adds, "as far as it is owing to the sun's motion it may be regarded as a kind of secular parallax, which, if the annual parallax of a few of the stars should some time or other be discovered, and the quantity and direction of the sun's motion should be discovered also, might serve to inform us of the distance of many of them, which it would be utterly impossible to find out by any other means."

Lambert's argument, that the fact of the sun's rotatory motion about its axis affords a presumption of its translation in space, was adopted by Lalande, who, in a memoir presented to the Academy of Sciences of Paris in 1776, concludes that inasmuch as the application of any force causing a body to turn about its centre cannot fail to displace the centre, the sun must necessarily have a real motion in absolute space. This argument will not be allowed to have much weight when it is considered that the sun's rotatory motion *may*, and probably does, proceed from causes wholly different from an eccentric impulsion.

Whatever degree of probability such *à priori* considerations may be supposed to give to the hypothesis of the sun's proper motion, it is evident that something more is necessary to render the hypothesis of any practical importance. The first astronomer who attempted to prove the existence of the sun's motion from observations,

and ventured to assign the precise point in the heavens towards which our system is actually borne, was Sir WILLIAM HERSCHEL. The paper containing this investigation was published in the Philosophical Transactions for 1783; and it is remarkable not only by reason of its giving the first determination of the kind, but on account of the confirmation which the result, though deduced from very insufficient data, has received from subsequent investigations—a circumstance, however, by no means rare in respect of the cosmical speculations of Sir W. HERSCHEL.

After some general considerations respecting the *à priori* probability of the sun's proper motion, Sir W. HERSCHEL, in the paper alluded to, proceeds to describe the phenomena to which it would give rise, namely, a general parallactic motion of the stars, the amount of which, in respect of any star, will depend both on the star's distance from the sun and its situation in the sphere with reference to the point towards which the sun is moving. It is manifest that if we suppose the sun to move in the direction of any assumed point, all the stars which are near enough to our system to be sensibly affected by such a motion, will appear to move towards the point diametrically opposite; and that on one side of the sphere all the right ascensions will appear to increase, while all those on the other side will appear to diminish. He selected seven stars—Sirius, Castor, Procyon, Pollux, Regulus, Arcturus, and α Aquilæ—all of which appeared from comparisons made by Dr. MASKELYNE to have proper motions in right ascension, and two of them—Sirius and Arcturus—also in declination; and finding that the right ascensions of all of them, with the exception of α Aquilæ, appeared to diminish, he assumed the direction of the sun's motion to be from a point "somewhere not far from the 77th degree of right ascension to its opposite 257th degree," the effect of which would be to produce apparent changes of right ascension agreeing with the observed; and he adds, "supposing the sun to ascend at the same time towards some point in the northern hemisphere, for instance towards the constellation of Hercules, then will also the observed change of declination of Sirius and Arcturus be resolved into a single motion of the solar system." In order to test this conclusion he selected twelve stars, quoted by LALANDE from MAYER's table above referred to, the motions of which were assigned both in right ascension and declination; and adding the motions in right ascension of three other stars, he thus obtained twenty-seven changes of position to be accounted for by the hypothesis. By assuming the sun's motion to be directed towards a point "somewhere near λ Herculis," he found that twenty-two of these motions were satisfied, there being only two exceptions in right ascension, and three in declination. The point thus indicated is situated at 257° of right ascension, and 25° of north declination; but he observes that with respect to the changes of declination the point λ Herculis is not, perhaps, the best-selected, as a somewhat more northern situation may agree better with the changes of declination of Arcturus and Sirius, "which capital stars," he thinks, "may be the most proper to lead us in this hypothesis."

In a *Postscript* to his paper, Sir W. HERSCHEL compares the above conclusion with

the proper motions of the other stars in MAYER's table, and shows that, out of forty-four stars, the observed motions of thirty-two agree with the hypothesis, while those of the remaining twelve cannot be accounted for by it, and "must therefore be ascribed to a real motion of the stars themselves, or to some still more hidden cause of a still remoter parallax."

It will be remarked that the above result was arrived at without the aid of any calculation whatever, nor does it appear that the precise direction of the apparent motion of any of the stars was ascertained or taken account of. The author considered merely the changes in right ascension and declination, and gave such a direction to the solar motion as would produce corresponding changes in those two directions in the greatest number of instances, without reference to their relative amounts, or attempting to produce an exact coincidence of the hypothetical and apparent directions in any particular case. Nor did he pretend to assign the point towards which the sun's motion is directed with any precision; "it is somewhere near λ Herculis, but may be somewhat more to the north."

In the same year (1783) in which Sir W. HERSCHEL's paper appeared in the Transactions, PREVOST communicated the results of a similar inquiry to the Berlin Academy in a memoir which was published in the Nouveaux Mémoires of that Society for 1781. PREVOST's investigation was also grounded on the proper motions given in MAYER's table. After stating the opinion of MAYER that the observed motions could not be explained on the hypothesis of the motion of the solar system, he remarks, that on examining the table under every point of view, he had come to an opposite conclusion, and found that it did in fact afford indications of such a motion, although the true motions of the stars, or, perhaps, some other cause, occasioned exceptions. He then selects, from MAYER's list, twenty-six stars whose variations of position exceeded 14″ in right ascension or declination, and from a comparison of the whole concludes that the apparent motions indicated by the table would be most nearly represented by supposing the sun's motion to be directed towards that point of the heavens of which the right ascension is 230°, and the declination 25° north,—a conclusion which agrees with that of Sir W. HERSCHEL in respect of declination, but differs from it about 27° in right ascension. The agreement of the individual observations with this result he considered was sufficient to render it probable, first, that the solar system is actually moving towards the point indicated, and, secondly, that at the present time the stars which are the nearest to the sun are Sirius, Procyon and Arcturus; and he thought the observations also afforded grounds for conjecturing that the sun may be describing, *in antecedentia*, an orbit about Arcturus, or at least about a centre of gravity common to those brilliant stars which occupy the quarter of the heavens in which the right ascensions appear to diminish, such as Arcturus, Regulus, Procyon, Sirius*.

* Among the inferences drawn by PREVOST from the hypothesis of the sun's proper motion, the following may be remarked:—supposing comets to be formed of matter existing beyond our system, but projected so as

A third deduction from MAYER's table was made by KLUGEL, in the Berlin Ephemeris for 1789. After giving formulæ for determining from the observed variations in the positions of the stars the direction of the sun's motion in space, he applies them to the proper motions given in the table, and finds the pole towards which the sun's motion is directed to be at the point of which the right ascension is 260° and north declination 27°. This differs from Sir W. HERSCHEL's determination only by 3° of right ascension, and 2° of declination.

Although the general agreement of these three results was calculated to draw the attention of astronomers to the subject, and served, at least, to give a certain plausibility to the hypothesis, no further addition was made to the data of the problem till the publication, in 1790, of Dr. MASKELYNE's table of the proper motions of thirty-six stars. This table, which furnished much more certain data than had previously existed, gave occasion to a second elaborate memoir by Sir WILLIAM HERSCHEL, which appeared in the Philosophical Transactions for 1805.

The mode of proceeding employed by Sir W. HERSCHEL in this memoir merits attention. Having computed from the observed variations of right ascension and declination the apparent direction of the proper motion of each of the stars, he traced on a celestial globe the great circles in which they were contained. On the supposition that the variations in question were parallactic motions caused by the translation of the sun, it was evident that all the great circles containing them would intersect each other in the same two opposite points of the sphere. Now of the intersections thus formed by taking the stars in pairs, he found ten made by six stars of the first magnitude to be contained within a very limited portion of the heavens about the constellation Hercules, while (he remarks) "upon all the remaining surface of the globe there was not the least appearance of any other than a promiscuous situation of intersections, and of these only one was made by arches of principal stars." The six stars which gave the contiguous intersections were Sirius, Arcturus, Capella, Lyra, Aldebaran and Procyon. But six stars combined by pairs give fifteen intersections; of these, therefore, five were rejected, that is to say a third of the whole, as not agreeing with the hypothesis. He then computes, by a trigonometrical calculation, the exact situations of the points of intersection of the ten arches, and (taking the points from which the stars appeared to recede) found them to be all included between 235° and 290° of right ascension, and between 17° and 58° of north declination. He then takes into account the motions of three other large stars, of the second order, and, on combining them with those of the former six, found out of the whole number of new intersections fifteen which agreed with the former in "pointing out the same part of the heavens as a parallactic centre." The positions of these fifteen new points were not calculated, but determined graphically; he conceived, however, they might be depended on as true to one degree of the sphere.

to come within the sphere of the sun's attraction, ought it not (he asks) to happen that more comets will appear in the quarter of the heavens towards which the sun is advancing than the opposite quarter?

The intersections thus found, although lying in the same quarter of the heavens, were not confined within a very narrow space, and in order to obtain a precise result, he proceeds as follows. Confining his attention to the six stars above named, he found the sum of their annual apparent motions in space to be $5''\cdot3537$. Now, assuming the star λ Herculis (as determined in his first paper) to be the point towards which the sun is moving, he computes the angle included between the great circle of the sphere which passes through this point and the star, and the great circle in which the star's motion takes place according to the comparison of the catalogues; he then multiplies the apparent quantity of the annual proper motion of the star by the cosine and sine respectively of this angle, whereby the apparent proper motion is resolved into two parts,—one in the direction in which the star would appear to move in consequence of the hypothetical motion of the sun, and the other at right angles to that direction. The first of these may be ascribed to the motion of the sun; the second must be regarded as due to the true proper motion of the star. Adding, therefore, into one sum the former of these resolved parts for each of the six stars, and deducting the sum from the sum of the observed annual motions, the latter sum was reduced from $5''\cdot3537$ to $2''\cdot2249$. By assuming another point in the same constellation as the apex of the sun's motion, the sum of the annual proper motions of the six stars, in the direction perpendicular to that resulting from the hypothesis, was further reduced to $1''\cdot4594$; and after some other trials, he ultimately fixed upon the point (near 34 Herculis) whose right ascension was $245° 52' 30''$, and north declination $49° 38'$, by which the sum of the true annual proper motions of the six stars was reduced to $0''\cdot9559$. He concluded that this point must be very near the truth, inasmuch as "the alteration of a few minutes in right ascension or north polar distance, either way, would immediately increase the required real motion of our stars."

This determination of the position of the solar apex differs from that which was given in Sir W. Herschel's former paper by about $11°$ of right ascension, and $24° 38'$ of declination. It rests, however, on the proper motions of only six stars, and, therefore, notwithstanding the greater probable accuracy of the observations, and the more elaborate process of calculation by which it was arrived at, it is probably not of greater intrinsic value than the first. The principle on which it is based, namely, the supposition that the sum of the true proper motions of the stars is a minimum, and consequently that the direction to be assigned to the sun's motion must be that which will account for the greatest amount possible of the observed motions, was objected to by Burkhardt, on the ground that there is no more reason for supposing the sum of the true proper motions to be a minimum than a maximum, excepting on the hypothesis that the stars are more inclined to rest than to motion. But this objection seems to imply some misapprehension of the problem under consideration. No hypothesis respecting the disposition of the stars to rest or to motion is involved. The apparent proper motions are the results of the comparison of the catalogues, and

the question proposed by Sir W. HERSCHEL was simply to determine the point towards which the sun must be supposed to move, in order that, after deducting the parallactic effect, the amount of the residual motions might be the least possible. BURKHARDT's memoir was published in the Connaissance des Temps for 1809. It contains formulæ for the solution of the problem, with their application to several of the stars in MASKELYNE's catalogue; but he found little accordance among the results, and concluded that we are not yet in possession of a sufficient number of facts to decide on the direction of the sun's motion.

BIOT, in the Additions to his Astronomie Physique, also considered the question of the translation of the planetary system, and gave formulæ for determining the right ascension and declination of the solar apex. He computed the intersections of the great circles containing the arcs described by eight stars, viz. Aldebaran, Capella, Sirius, Procyon, Pollux, Arcturus, α Lyræ, and α Aquilæ, the proper motions of which were given by ZACH from a comparison of BRADLEY's places with those of MASKELYNE, and also of the catalogues of MAYER and PIAZZI. If the apparent motions depended solely on the displacement of our system, the intersections would, of course, all be found at the same two points of the sphere; but he found the discrepancies to be so great that he considered them to be irreconcileable with the supposition of their dependence on any systematic motion or common cause. "The examination," he remarks, "of all these irregularities shows that the stellar motions hitherto observed are not subject to any law, and that it would be in vain to attempt to reconcile them by supposing them all to be directed towards the same pole. Hence it becomes infinitely probable that such of these motions as are well determined are due, in part, to a real displacement of the stars themselves, and not to that of our system. With respect to those whose proper motions are less certain, not only does their want of precision prevent us from concluding from them the direction of the motion of our own system, but their comparison does not even afford any indication which can lead to the inference that it is in motion at all."

In the 12th section of his Fundamenta Astronomiæ, BESSEL has given an elaborate investigation of this question, founded on a much larger number of proper motions (and probably better determined) than had previously been brought to bear on the inquiry. On comparing the catalogue deduced from BRADLEY's observations with that of PIAZZI, he found seventy-one stars having a proper motion of not less than $0''\cdot5$ annually in the arc of a great circle, and computed the positions of the great circles in which the apparent motions are contained. But even from this large number he obtained no satisfactory or conclusive result. The investigation, he remarks, did not confirm HERSCHEL's supposition of the sun's motion towards the constellation Hercules, since many points on the sphere, very remote from each other, and even diametrically opposite, may be assigned which are situated in the direction of the motion of many stars; but whatever point may be taken, there will always be found so many proper motions evidently receding from it, that no sufficient reason

will remain for preferring one point to another. And he concluded that a very long time must elapse before any proficiency would be made in the theory of the proper motions of the stars.

The opinion of Bessel, now quoted, appears to be that which, until lately at least, has been generally entertained by astronomers; but on attentively considering the nature of the question, it will soon be seen that none of the methods of investigation yet alluded to can be considered as capable of leading to an entirely satisfactory conclusion. They are all founded essentially on the principle of determining the apex of the sun's motion from the apparent motions of single pairs of stars; and, with the exception of Bessel's, all the results which had been given were deduced from a very small number of proper motions. Now, it is a very improbable supposition that the stars are subject to no variations but such as depend on the motion of the sun. We must suppose them to have true proper motions, producing apparent effects at least equal in amount to those which are supposed to be produced by the sun's displacement. Assuming, then, that the stars themselves are in motion as well as the sun, and that they move in all directions, the appearances will necessarily be of a very complicated nature. The proper motions of some stars will conspire with that of the sun, and increase the apparent change of position. In other cases they will be contrary to that of the sun, and the apparent effect will be that which is due to the difference of two real motions. In general the directions of the true and parallactic motions will be inclined to each other; but in all cases the difference given by the comparison of the catalogues will be compounded of the effect of the real motion of the star and the effect of the sun's displacement. Hence it is manifest that the fact of proper motions being observed to take place in all directions, is in no way inconsistent or incompatible with an apparent general drifting of the stars towards one particular region; and the problem to be solved is to separate, if possible, the general effect produced by the sun's displacement from the complicated effects caused by the motions of stars in every direction with which it is entangled and mixed up. Now it is easy to see that a question of this kind cannot be solved by taking account of only a small number of proper motions. A very considerable number must be employed; and, indeed, in order that the solution may be satisfactory, regard must be had to every star without exception of which the proper motion has been determined with sufficient certainty. It is also necessary that the investigation be conducted in such a manner that every observed displacement shall contribute, according to its weight, to the general result; and the probable error of the result must likewise be determined in order that the relative probabilities of results obtained from different hypotheses respecting the direction of the sun's motion, may be submitted to exact comparison. In this manner it will be seen whether, as Bessel and others inferred from particular cases, numerous points may be assumed towards any one of which the sun may be supposed with equal probability to be advancing, or whether there is so great a preponderance of observed motions towards one particular region

as to warrant the assumption of a systematic origin, or make evident the operation of a common cause. This mode of considering the question was first adopted by ARGELANDER, in the memoir alluded to at the commencement of this paper.

ARGELANDER's investigation, as already stated, is founded on the proper motions of 390 stars, determined by a comparison of their mean places in 1755, according to BESSEL's reduction of BRADLEY's observations, with their mean places in 1830, as given in his own catalogue*, deduced from observations made by himself at Abo, the interval between the epochs being seventy-five years. In this investigation every star was included which appeared, on comparison of the two catalogues, to have undergone a change of position to the extent of $7''\cdot5$, or to have an annual proper motion amounting to $0''\cdot1$ in space. By reason of the excellence of both catalogues, the long interval between their respective epochs, and the very considerable number of stars employed, the result must be considered as by far the most satisfactory that had yet been given.

The method of calculation employed by ARGELANDER may be described generally as follows:—1. From the variation in right ascension and declination given by the comparison of the catalogues, the angle (ψ) is computed which the apparent path described by the star makes with the circle of declination. 2. A point (Q) is assumed as the apex of the sun's motion, and the direction in which the star would appear to move (if it had no real motion of its own) in consequence of the motion of the sun, is computed from the position of the star and the assumed position of the point Q, and expressed in terms of the angle (ψ') which it makes with the declination circle. 3. The trigonometrical value of ψ' is differentiated on the supposition that the right ascension (A), and declination (D) of the point Q are variable quantities, and in the resulting expression the numerical value of the difference of the angles ψ and ψ' is substituted for $d\psi'$, by which means an equation is obtained in which there are only two undetermined quantities, viz. dA and dD. Each star furnishes a similar equation; and as the effect of the real motion of the sun on the apparent displacement of any star is proportional to the sine of the star's distance from the apex of the sun's motion, each equation is multiplied by the corresponding sine of this distance, whereby they are all reduced to the same degree of precision. 4. The equations are then solved by the method of least squares, and the resulting values of dA and dD applied as corrections to the assumed values of A and D, which determine the situation of the point Q. With the corrected values of A and D thus obtained, the angles ψ' may be recomputed, and after one or two repetitions of the same process, values of A and D will be obtained giving the position of Q which most nearly represents the whole of the observations.

The effect of the sun's displacement on the apparent proper motion of a star is inversely proportional to the distance of the star from the sun; but this distance

* DLX Stellarum fixarum positiones mediæ ineunte anno 1830. Ex observationibus Aboæ habitis deduxit . . . . . . Fr. ARGELANDER. Helsingforsiæ, 1835. 4to.

being entirely unknown, no account can be taken of it excepting upon some assumption more or less arbitrary. Argelander assumes as a probable hypothesis, that those stars which have the largest proper motions are the nearest to our system, and introduces the condition of relative proximity by dividing the stars upon which his calculation was made into three classes, and giving different weights to the equations belonging to the different classes, all the stars in the same class being assumed to be at the same mean distance. The first class contained twenty-one stars, having proper motions exceeding one second of arc annually; the second contained fifty stars whose annual proper motions are between $1''\cdot0$ and $0''\cdot5$; and the third the remaining 319 stars, the annual proper motions of which were included between $0''\cdot5$ and $0''\cdot1$. The partial results deduced from each class presented a nearer agreement than was, perhaps, to be anticipated from the nature of the question. In giving an account of his memoir in No. 363 of the Astronomische Nachrichten, Argelander corrects some errors of calculation which had escaped detection in the original paper, and states the most probable values (with their probable errors) of the right ascension and declination of Q, as resulting from the combination of the whole of the equations of condition, to be as follows:—

$$A=259^\circ\ 47'\cdot6\pm3^\circ\ 18'\cdot6,\quad D=+32^\circ\ 29'\cdot5\pm2^\circ\ 13'\cdot5,$$

for 1792·5 (the mean epoch of the catalogues), or

$$A=259^\circ\ 51'\cdot8,\quad D=+32^\circ\ 29'\cdot1,$$

when reduced to the beginning of 1800.

This result differs very considerably from that which was obtained by Sir W. Herschel in his paper of 1805, viz. $A=245^\circ\ 52'\ 30''$, $D=+49^\circ\ 38'$, but approximates nearly to the determination in the paper of 1783; the difference from the latter being less than 3° in right ascension, and about $7\frac{1}{2}^\circ$ in declination.

In order to give an idea of the probable accuracy of this result, Argelander deduces the following conclusions. If with the point Q thus found as a centre, and a radius containing $3^\circ\ 45'\cdot7$, a circle be described on the sphere, the wager is 1 to 1 that the sun's motion is directed to some point within this circle; 14 to 3 that it is directed to some point within a circle having the same centre and a radius of $7^\circ\ 31'\cdot4$; 89 to 4 that it is directed to a point within a circle having the same centre and a radius of $11^\circ\ 17'\cdot1$; 142 to 1 that the point will be within a circle having the same centre and a radius containing $15^\circ\ 2'\cdot8$; and if we increase the radius to $18^\circ\ 48'\cdot5$, the wager will be more than 1341 to 1 that the point Q will lie somewhere within that circle.

In No. 398 of the Astronomische Nachrichten, Argelander returns to the subject a third time, and gives another determination of the direction of the solar motion, calculated by Lundahl from a different set of stars. The Abo catalogue does not contain the whole of Bradley's stars given in the Fundamenta Astronomiæ, and on comparing the latter work with Pond's catalogue of 1112 stars (reduced to the begin-

ning of 1830), LUNDAHL found 147 not included in ARGELANDER's investigation, whose proper motions appeared to be not less than $0''\cdot09$ of space annually. Having first recomputed the places of those stars with a more exact value of the precession, and applied to POND's observations the correction necessary to render them strictly comparable with those of ARGELANDER, LUNDAHL assumed the apex of the sun's motion to be situated at the point indicated by ARGELANDER's investigation, and calculated the value of ψ' for each star on this assumption. Comparing the directions thus obtained with those of the apparent motions, and forming the equations of condition according to the method of ARGELANDER, the resulting values of the right ascension and declination of Q were found to be

$$A=252^\circ\ 24'\cdot4\pm5^\circ\ 25'\cdot3\ ;\quad D=+14^\circ\ 26'\cdot1\pm4^\circ\ 29'\cdot3.$$

This result differs from that of ARGELANDER more than 8° in right ascension, and about 17° in declination; and the corrections of the assumed values are far beyond the limits of the probable errors assigned by ARGELANDER. By reason, however, of its smaller weight, it does not, when combined with the former determinations, materially alter the probable situation of the point Q. Combining it with the results of each of his three classes, with due regard to their relative weights, ARGELANDER gives the following values of the coordinates of Q as the most probable result of the whole of the observations:—

$$A=257^\circ\ 49'\cdot7\pm2^\circ\ 49'\cdot2\ ;\quad D=+28^\circ\ 49'\cdot7\pm1^\circ\ 59'\cdot8.$$

A still more recent attempt to assign the position of the apex of the sun's proper motion has been made by OTTO STRUVE, the results of which are given in a paper published in the Petersburg Memoirs (tome 5) for 1842. This investigation is grounded on the proper motions of about 400 stars, as determined by a comparison of their mean places in 1755, according to BESSEL's catalogue, with their positions in 1825 deduced from observations made at the Dorpat Observatory. Of the whole number of stars employed, only 134 are included among those from which ARGELANDER's result was deduced, so that about 260 additional proper motions are brought to bear on the hypothesis. The mode of investigation is different in several respects from that which has been described. Assuming the direction of the sun's proper motion to be determined, the proper motions indicated by the comparison of the catalogues are manifestly functions of the constant of precession used in reducing BRADLEY's places to 1825, and of the quantity of the solar motion. From the equations of condition furnished by the observed variations of right ascension and declination, he determines the precession and the angular motion of the sun (which, as seen from the mean distance of stars of the first magnitude, he finds to be $0''\cdot339$ in a year, with a probable error of $0''\cdot025$), and having substituted these values in the equations of condition, he employs the residual errors in forming a new system of equations which serve to determine dA and dD, the corrections in right ascension and declination of the assumed direction of the solar motion. The directions of the

apparent proper motions were not computed, but the variations in right ascension and declination expressed separately in terms of the assumed values of A and D and the star's place, so that each star furnishes two independent equations. For determining the relative weights of the equations, he adopts the hypothesis that the distances of the stars are inversely as their apparent magnitudes; and, dividing all the stars from the first to the seventh magnitude inclusive into twelve classes, he assumes, on grounds given by the elder STRUVE, in the Introduction to his catalogue of double stars, the mean distance of those in the first class =1, of those in the second =1·71, and so on to the twelfth class, or seventh magnitude, for which the mean distance becomes 11·34. Weights depending on those distances were assigned to the equations furnished by the stars in each of the twelve classes, and the values of dA and dD deduced. The result gave the position of Q, for 1790, as follows:—

$$A=261° \ 21'{\cdot}8 \pm 4° \ 49'{\cdot}9; \quad D=+37° \ 36'{\cdot}0 \pm 4° \ 11'{\cdot}8.$$

For the sake of comparison, I here subjoin the results of the several investigations of ARGELANDER, LUNDAHL, and OTTO STRUVE.

Position of the apex of the sun's proper motion for 1792·5,

		A=	D=	
ARGELANDER	I.	256° 25′·1±12° 21′·3	+38° 37′·2±9° 21′·4	(21 stars).
ARGELANDER	II.	255° 9′·7± 8° 34′·0	+38° 34′·3±5° 55′·6	(50 stars).
ARGELANDER	III.	261° 10′·7± 3° 48′·9	+30° 58′·1±2° 31′·4	(319 stars).
LUNDAHL	IV.	252° 24′·4± 5° 25′·3	+14° 26′·1±4° 29′·3	(147 stars).
O. STRUVE	V.	261° 23′·1± 4° 49′·9	+37° 35′·7±4° 11′·8	(392 stars).

From these five determinations O. STRUVE deduced the following mean result:—

$$A= \ 259° \ 9'{\cdot}4, \text{ with a probable error } =2° \ 57'{\cdot}5.$$
$$D=+ \ 34° \ 36'{\cdot}5, \text{ with a probable error } =3° \ 24'{\cdot}5.$$

The proper motions on which the following investigation is grounded are deduced from a comparison of the mean positions of eighty-one stars in the southern hemisphere, as observed by Mr. JOHNSON and the late Professor HENDERSON, with the positions assigned to them in the catalogues of LACAILLE and BRADLEY. Every star, without exception, has been included, which, from the differences of right ascension and declination given in the two recent catalogues, appears to have a proper motion amounting to 0″·1 in space, or upwards, annually.

Mr. JOHNSON's catalogue, which was published in 1835, gives the mean positions of 606 stars, observed by him at St. Helena, and reduced to the beginning of 1830. Of these stars a considerable number are contained in LACAILLE's catalogue, originally published in the Astronomiæ Fundamenta (1757), and recently by Mr. BAILY in vol. v. of the Memoirs of the Royal Astronomical Society. The epoch of LACAILLE's catalogue is 1750, so that the interval is eighty years. In order to compare the two catalogues, Mr. JOHNSON reduced the positions given by LACAILLE to the epoch 1830, by applying

the precession due to the middle epoch, 1790; and on examining the proper motions of the stars so compared, I have found fifty-six which appear to have changed their positions $8''\cdot0$ or upwards in the arc of a great circle, or to have an annual proper motion of not less than $0''\cdot1$. To these have been added five others whose annual proper motions appear to be somewhat less than $0''\cdot1$, according to Mr. Johnson's observations, but to amount to that quantity according to Henderson's determination compared with that of Lacaille. The whole number of stars, therefore, included in the present inquiry, whose proper motions are deduced from the comparison of the observations of Mr. Johnson with those of Lacaille, is sixty-one.

Henderson's catalogue contains the mean right ascensions and declinations of 172 of the principal southern stars, being part of a very much larger number observed by him during his short residence at the Cape in 1830 and 1831, while in charge of the Government Observatory established in that colony, the reduction of which unfortunately he did not live to complete. The declinations were published in 1838, in vol. x. of the Memoirs of the Royal Astronomical Society, and the right ascensions in vol. xv. of the same series, which appeared only last year (1846). In the latter volume he gives a list of fifty-two stars which appear to have a proper motion of not less than $0''\cdot1$ annually, deduced in thirty-six cases from a comparison of his own observations with those of Lacaille, and in the remaining sixteen cases with those of Bradley. Henderson's mean places are for the beginning of 1833; so that the interval is eighty-three years in the case of comparison with Lacaille, and seventy-eight years in the case of comparison with Bradley. The whole of the thirty-six stars compared with Lacaille, are contained in Mr. Johnson's catalogue, but there are four of them in respect of which the comparison is not given by Mr. Johnson, and which, therefore, are not included among the sixty-one above referred to. Henderson's catalogue, therefore, gives twenty additional stars, so that on the whole the number taken into account is eighty-one; namely, sixty-five whose proper motions depend on Lacaille's observations, and sixteen on the observations of Bradley.

For the purpose of deducing the direction of the apparent proper motion from the observed variations of right ascension and declination, as well as for determining the hypothetical direction, every star has been referred to its mean position for 1790. In the case of Mr. Johnson's stars, this reduction has been made by taking the mean right ascension and the mean declination of the two compared catalogues. In respect of the stars in Henderson's catalogue, those which are compared with Lacaille's places were first reduced to 1830 by applying the precession given in the catalogue, and the mean then taken between these reduced places and the places of Lacaille; and in the case of those compared with Bradley's catalogue, the mean of the two catalogues gave the positions for 1794, from which they were reduced to 1790, by applying the precession for that epoch. With respect to the stars common to the catalogues of Mr. Johnson and Henderson, the annual variation both in right ascen-

sion and declination was first deduced from the comparison of each catalogue with LACAILLE's places, and the mean of the two comparisons then taken and made use of in the subsequent calculations.

Having thus obtained the places of the stars for the mean epoch 1790, and the annual variations in right ascension and declination being found by dividing the differences of the catalogues (expressed in seconds of arc) by the number of years in the interval, the angle ψ, which the apparent path of the star makes with the circle of declination, was computed. This angle determines the position of the great circle of the sphere in which the apparent motion takes place.

The next step in the process is to compute the angle ψ', or the direction in which the star would appear to move in consequence of the translation of the sun towards an assumed point Q. With respect to this point, or apex, the position which may be regarded as the most probable is, perhaps, that which was deduced by OTTO STRUVE from the five determinations above given; but the object here was not to choose the point which has the greatest probability in its favour, but that which appeared the most likely to satisfy the present observations. Now, on examining the apparent motions of the stars under consideration, it was easy to see that the apex must have a considerably greater declination than that which was assigned to it by LUNDAHL. OTTO STRUVE's result, on the other hand, which differs from the mean in the opposite direction, appeared to me to be less trustworthy from the manner in which it was deduced. I therefore assumed, as the apex of the solar motion, the point determined by ARGELANDER, though, as it turns out, the motions in declination would have been somewhat better satisfied by assuming the mean of all the results as given by OTTO STRUVE. Its position for 1790 (the mean epoch of the catalogues) is

$$A=259° \ 46'{\cdot}2, \ D=+32° \ 29'{\cdot}6^*.$$

From these values of A and D the angle ψ' was computed for each of the eighty-one stars separately. The results, as well as the values of ψ, and the differences $\psi-\psi'$, will be found in a table hereto subjoined.

Before proceeding to state the results obtained from the equations of condition, it will be worth while to examine the presumptions for or against the hypothesis deducible from the comparison of the directions of the apparent proper motions of the different stars, and the directions of the parallactic motions which would result from the motion of the sun towards the assumed point Q.

First, with respect to the observed variations of right ascension: if we conceive the celestial sphere to be divided into two hemispheres by a great circle passing through

* This is the position according to the values of the right ascension and declination given by ARGELANDER in No. 363 of the Astronomische Nachrichten, but in a subsequent number of the same work (No. 398) it is stated that an error of calculation had been committed the correction of which would have given the position of Q for 1792·5 as follows: Æ$=260° \ 51'$, Dec. $=+31° \ 17'$. The correction was not observed till after the values of ψ' had been calculated for all the stars; but for the present purpose the difference is manifestly of no importance.

Q and perpendicular to the equator, the effect of the sun's motion towards Q would be to increase the right ascensions of all the stars in one of the hemispheres, and to diminish the right ascensions of all those in the other. Now, out of the whole of the stars compared, thirty-five are situated in the hemisphere in which the right ascensions should increase according to the hypothesis, and of these there are twenty-three whose right ascensions have actually increased, and twelve whose right ascensions have diminished; that is to say, there are twenty-three instances favourable to the hypothesis, and twelve unfavourable. In the other hemisphere there are forty-six stars; and of these the number whose right ascensions have diminished, agreeably to the hypothesis, is thirty-nine, and the number whose right ascensions have increased, contrary to the hypothesis, is seven. Hence it appears that in respect of the eighty-one stars included in the inquiry the observed motions in right ascension are favourable to the hypothesis in sixty-two instances, and unfavourable in nineteen. Allowing the same weight to each instance, the wager is therefore sixty-two to nineteen, or somewhat more than three to one in favour of the hypothesis of a common tendency towards a determinate region.

Secondly, with respect to the observed variations of declination. In all cases in which the angle ψ' is less than 90°, or greater than 270°, the effect of the sun's motion in the direction of the assumed point Q, is to bring the apparent place of the star towards the north, so that its declination (which is south in all cases) should appear, on comparison of the catalogues, to have diminished; and in all cases in which ψ' is greater than 90° and less than 270°, the variation in the place of the star should be towards the south, and the declination should increase. Now, in the first case there are fifteen stars, and of these ten have advanced towards the north, agreeably to the hypothesis, and five towards the south, contrary to the hypothesis. In the second case there are sixty-six stars; and of these the observed motion of fifty-three is towards the south, agreeing with the hypothesis, and of thirteen towards the north, contrary to the hypothesis. On the whole, therefore, in respect of declination, there are sixty-three instances favourable to the hypothesis and eighteen unfavourable; and the wager is seven to two in favour of the hypothesis. It may be added that there are only three stars out of the whole number (Nos. 18, 34 and 36 in the subjoined table) whose observed proper motions are contrary to the hypothesis both in right ascension and declination.

Another inference may be drawn from the comparison of the angles ψ and ψ'. If the observed changes of position were wholly independent of the sun, all directions would be equally probable, and it would be an even wager that the difference between ψ and ψ' would be less or greater than 90° in any case, since all possible values of that difference lie between 0 and 180°. But there are only ten instances, out of eighty-one, in which the difference exceeds 90°.

From this general agreement of the hypothetical and observed motions, a strong presumption is raised in favour of the hypothesis; for it can scarcely be supposed

that the agreement would hold good in so great a majority of instances if it were purely the effect of chance. But a much more certain conclusion will be arrived at from the combination of the whole of the observations by the method of least squares.

The method of forming the equations of condition has already been explained generally, and the formulæ for computation will be given in subsequent paragraphs; but as some of the stars are more favourably circumstanced than others for determining the question at issue, it becomes necessary, before proceeding with the solution, to assign weights to the equations, in order to reduce them all to the same precision, and obtain the most probable values of the corrections to be applied to the assumed position of the solar apex. For this purpose some special considerations are required.

Admitting the hypothesis of the sun's motion, it can hardly be supposed that any star is absolutely at rest. The apparent motion of a star, therefore, as it is made known to us by a comparison of observations, is the effect of the combined motion of the sun and the star. Now, with respect to the true proper motion, we are in ignorance of all the circumstances by which its apparent or visible effect is modified. We know nothing whatever respecting the magnitude or nature of the orbit described by the star, or the absolute velocity with which it moves. Hence it is necessary to assume that all the stars move with the same absolute velocity, in which case (putting the sun's motion out of consideration) the apparent velocity will be inversely as the distance. But we are equally ignorant of the relative distances, and are therefore reduced to the necessity either of disregarding the distance altogether, or of making some precarious assumption respecting it,—for instance, that the distances of the different stars are inversely proportional to their magnitudes (as in the method of Otto Struve), or inversely as the quantities of the apparent proper motions. In the present inquiry no greater probable accuracy could be obtained by the adoption of either of these assumptions, and consequently errors to which differences of distance as well as of absolute velocity give rise, are regarded as constant. The only remaining circumstance by which the apparent effect of the true proper motion is modified is its direction; and as there is no *à priori* reason for assuming that a star is more likely to move in one direction than another, all directions must be regarded as equally probable. The conclusion, therefore, is, that in respect of the true proper motions *inter se*, we have no sufficient grounds for making any distinction as to the relative precision of the results given by different stars, so that the errors arising from this cause must be treated as accidental errors of observation, and all the equations be allowed to have the same weight.

With respect to the part of the apparent motion depending on the displacement of the sun, the case is different, inasmuch as the parallactic effect depends not only on the distance of the star, but also on its situation with respect to the apex of the sun's motion. The effect of the sun's motion on the observed position of a star (as will be shown more particularly further on) is directly as the sine of the star's distance from

the point Q towards which the sun is moving; and hence, in order that all the equations may have the same weight, each must be multiplied by the sine of that distance. In other words, if $\varepsilon(\Psi)$ denote the probable error in the observed direction of the proper motion, or the probable value of $\psi-\psi'$, for a star at the distance of 90° from the point Q, then $\varepsilon(\Psi)$ will also be the probable value of $(\psi-\psi')\sin\chi$ for a star whose distance from Q is measured by the angle χ. Hence it follows that every value of $\psi-\psi'$ must be multiplied by $\sin\chi$.

The eighty-one equations of condition are given in an appended table. They are of the following form,

$$0=+a dA+b dD-n,$$

where a, b, and n are numbers deduced from the data, and dA, dD the quantities to be determined from the equations and applied as corrections to the assumed values of A and D. Forming the squares and products of these numbers, and adopting, according to the usual notation, (aa) to denote the sum of the squares of the coefficients of dA, (bb) the sum of the squares of b, (ab) the sum of the products of a and b, and so on, the following values are found:—

$$(nn)=178660{\cdot}4,\ (aa)=38{\cdot}5423,\quad (bb)=26{\cdot}7425,$$
$$(ab)=-5{\cdot}6852,\ (an)=-129{\cdot}462,\ (bn)=-105{\cdot}693,$$

and consequently the two following equations for determining dA and dD, viz.

$$0=+38{\cdot}5423dA-\ 5{\cdot}6852dD-129{\cdot}462,$$
$$0=-\ 5{\cdot}6852dA+26{\cdot}7425dD-105{\cdot}693,$$

the solution of which gives

$$dA=+4^\circ{\cdot}070 \text{ with the weight } 37{\cdot}333,$$
$$dD=+4^\circ{\cdot}817 \text{ with the weight } 25{\cdot}904.$$

On computing $\varepsilon(\Psi)$, or the probable value of $(\psi-\psi')\sin\chi$, from the appropriate formula of the method of least squares, we find $\varepsilon(\Psi)=31^\circ{\cdot}98$; whence, and from the above weights, the probable errors of dA and dD are respectively $5^\circ{\cdot}234$ and $6^\circ{\cdot}285$. The result, therefore, of the whole calculation from the assumed values of A and D, namely A=259° 46′·2, D=+32° 29′·6, gives the following values of A and D for the position of the point Q for the beginning of 1790,

$$A=263^\circ\ 50'{\cdot}4\pm5^\circ\ 14'{\cdot}0\ ;\ D=+37^\circ\ 18'{\cdot}6\pm6^\circ\ 17'{\cdot}1.$$

This result presents a very remarkable agreement with that obtained by Otto Struve from the Dorpat observations; the values of dA and dD are, however, somewhat greater than the probable errors of the hypothesis, according to the determination of Argelander.

Following out the principle of the method, the next step would be to recompute the angles ψ', and the equations of condition, with the values of A and D now found, so as to obtain a result having a smaller probable error; but, in the present case, the labour attending a new calculation (by no means inconsiderable) is altogether unnecessary, as will appear from the following considerations.

In the first place it is to be remarked, that the result has been deduced from all the stars whose annual proper motions were found to be not less than $0''\cdot 1$, without any selection or rejection on account of obvious discrepancies. But it is manifest that if there is observed a general tendency to motion in a determinate direction, while in one or two instances the motion is in a nearly opposite direction, the presumption will be that in such exceptional cases the disagreement arises from the circumstance that the parallactic motion is masked and concealed by the relatively greater proper motion of the star. In the second place, a few of the stars under consideration are very unfavourably situated for correct determination of the right ascension according to the method practised by LACAILLE, and also for correct comparison by reason of the uncertainty of the computed precession; and in such cases a disagreement with the general result naturally gives rise to a suspicion of error in the determination. Now there are two stars, β and γ^1 Octantis (Nos. 15 and 18 in the table) which are in those circumstances. The difference between the observed and hypothetical directions of their motion, or $\psi-\psi'$, is $158°\ 58'\cdot 0$ in the case of the first, and $179°\ 13'\cdot 0$ in that of the second, showing in the latter case an apparent motion almost directly opposite to the parallactic motion due to the hypothesis. Both stars are also situated within 8° of the pole, so that the determination of their right ascensions by LACAILLE'S method of equal altitudes must be liable to considerable uncertainty. For these reasons it may be concluded that the probable accuracy of the result will be increased by rejecting those two stars from the calculation.

Omitting, therefore, the two equations 15 and 18, the sums of the squares and products of the numerical quantities in the remaining seventy-nine are as follows:

$$(nn)=137120\cdot 9,\ (aa)=37\cdot 1490,\quad (bb)=26\cdot 7010$$

$$(ab)=-5\cdot 9259,\ (an)=+110\cdot 849,\ (bn)=-64\cdot 282\,;$$

which give the following solution,

$$dA=-\ 2°\ 41'\cdot 8\pm 4°\ 44'\cdot 9,$$

$$dD=+\ 1°\ 48'\cdot 5\pm 5°\ 36'\cdot 0$$

$$\varepsilon(\Psi)=\ 28°\ 25'\cdot 2.$$

Here the corrections, both in right ascension and declination, are considerably diminished, and it will be remarked that the former has changed sign and become subtractive instead of additive. The probable errors are also considerably reduced, and both corrections are now within the limits of the probable errors of ARGELANDER'S determination.

On applying the above corrections to the assumed values of A and D, we obtain, as the result of the calculation from seventy-nine stars,

$$A=257°\ 4'\cdot 4\pm 4°\ 44'\cdot 9,\ D=+34°\ 18'\cdot 1\pm 5°\ 36'\cdot 0.$$

Another omission will diminish the corrections and probable errors still further. One of the stars in HENDERSON'S catalogue (η Ophiuchi, No. 80 in the subjoined table)

appears to move in a direction nearly opposite to that of the parallactic motion resulting from the assumed hypothesis, the difference of the angles ψ and ψ' being in this case $173° 4'{\cdot}9$. If we reject this star also, on account of the great probability there is that the apparent motion is here due to the excess of the true proper motion of the star above the parallactic motion, we shall have from the remaining seventy-eight stars,

$$(nn)=120332{\cdot}5,\ (aa)=36{\cdot}4473,\ (bb)=26{\cdot}6875,$$
$$(ab)=-5{\cdot}8285,\ (an)=+2{\cdot}308,\ (bn)=-49{\cdot}213,$$

whence the following results,

$$dA=+0°\ 14'{\cdot}4\pm4°\ 31'{\cdot}4,$$
$$dD=+1°\ 53'{\cdot}8\pm5°\ 17'{\cdot}2,$$
$$\varepsilon(\Psi)=\ 26°\ 49'{\cdot}8.$$

Applying these corrections to the assumed values of A and D, the position of the point Q, for 1790, is found as follows :—

$$A=260°\ 0'{\cdot}6\pm4°\ 31'{\cdot}4,\quad D=+34°\ 23'{\cdot}4\pm5°\ 17'{\cdot}2.$$

These values of A and D are almost identical with those which were deduced by Otto Struve from the combination of his own result with those of Argelander and Lundahl.

From the near agreement of these results with the hypothesis, it is manifest that it would be an entirely useless labour to recompute values of ψ' from slightly altered values of the right ascension and declination of Q, the corrections of the assumed values being so far within the limits of the probable errors. So close a coincidence, whether accidental or otherwise, is not a little remarkable. In fact the southern stars would seem to accord with the hypothesis even better than those in the other hemisphere ; for the mean value of $(\psi-\psi')\sin\chi$, or $\varepsilon(\Psi)$, in respect of the whole of the stars, is less than the mean found by Argelander from his second and third classes ; and if we leave out the two stars above mentioned near the pole, it is less even than that given by his first class, the values for his three classes being respectively $31°\ 31'{\cdot}0$, $32°\ 36'{\cdot}6$, $35°\ 41'{\cdot}6$.

It is difficult to form a satisfactory estimate of the probable accuracy of the result of this calculation, as compared with the results of Argelander and Otto Struve. The number of stars, though not large, might perhaps be regarded as sufficient to render the result worthy of confidence if the proper motions in right ascension and declination indicated by the comparison of the catalogues could be safely relied on ; but, unfortunately, in the present case, the probable errors of observation are hardly susceptible of exact appreciation, and the result is of course affected by the uncertainty of the data. With respect to the two recent catalogues, there is, indeed, no difficulty, inasmuch as the probable errors can be estimated with sufficient precision. Mr. Johnson considers the probable error in right ascension of a position given by the mean of five observations to be $0^s{\cdot}034\times\sec\delta$ in time (δ being the declination of

the star), or $0''\cdot51 \times \sec\delta$ in arc. Taking, as a mean value, $\delta=45°$, this gives the probable error in right ascension $=0''\cdot72$. He also states the probable error in declination, from five observations, to be $0''\cdot35$, exclusive of the error occasioned by uncertainty of refraction. Assuming the average error in declination from all causes to be the double of this, or $0''\cdot70$, we shall have the probable error in the place of the star, in the arc of a great circle, $=\sqrt{\{0''\cdot72^2+0''\cdot70^2\}}=1''\cdot0$. In the case of HENDERSON's catalogue, the probable errors may be regarded as still smaller, owing to the superiority of the instruments of the Cape Observatory. But with respect to LACAILLE's observations, there is considerable uncertainty. His right ascensions were not determined, as in modern practice, by means of a transit instrument, but by the method of equal altitudes, with a 3-foot quadrant; and it is not certain whether the clock, on the accurate performance of which during the interval of the two observations of altitude the result mainly depends, was compensated for temperature. The declinations were observed with a 6-foot sector and a 6-foot sextant; and it is to be remembered that some of the most important elements of reduction—the aberration, nutation, refraction—were then imperfectly known. On the other hand, LACAILLE's well-known skill as an observer, the care he bestowed on the catalogue in the Fundamenta Astronomiæ, and the repeated examinations it has undergone by DELAMBRE and others, may be considered as rendering his positions trustworthy within limits which warrant their application to the purpose in hand. DELAMBRE, who had made extensive comparisons of LACAILLE's observations, estimated the probable error of one of his positions as double the probable error of one of BRADLEY's. But the probable error in declination of a star observed by BRADLEY is estimated by BESSEL at $0''\cdot7$; and the probable error in right ascension of an equatorial star, or, generally, the probable error of $\alpha\times\cos\delta$, is nearly the same as the probable error in declination; whence the probable error in the position of a star on the arc of a great circle may be taken at $\sqrt{2\times0\cdot7^2}=0''\cdot94$, or less than one second. Assuming, then, the probable error of one of LACAILLE's positions to be $2''$, and that of one of JOHNSON's (as above shown) to be $1''$, the probable error of the difference of the catalogues becomes $\sqrt{4+1}=2''\cdot236$; which divided by 80, the interval between the epochs, gives $0''\cdot028$ as the probable error of the annual proper motion deduced from the comparison of the two catalogues, so far as it depends on errors of observation. Hence it appears that a proper motion amounting to $0''\cdot1$ annually (the smallest which has been admitted in the present inquiry) considerably exceeds the probable errors of the catalogues, and consequently that the proper motions which have been under consideration not only have a real existence, but are determined with sufficient precision to give a result worthy of considerable confidence.

On the whole it may be said, that although the present result, if it stood by itself, would scarcely be considered as of sufficient weight to establish the fact and direction of the solar motion in space, yet coinciding as it does with those of ARGELANDER

and Otto Struve, it considerably increases the probability of the conclusions obtained by those astronomers. It is now shown that stars situated in every region of the heavens agree in their indication of a general motion directed towards one particular quarter; and as this agreement, not only of the results of different investigations, but of the great majority of the proper motions which have been ascertained and examined, cannot, on any reasonable supposition, be regarded as fortuitous, the inference is inevitable that they are, in part at least, systematic, and modified by the action of a general cause.

The proper motions which have been examined in this paper are not sufficiently numerous to warrant any speculation with respect to the nature of the path which the sun describes in space. Analogy leads us to infer that the sun must describe a curvilinear orbit, and if we suppose the orbit to be nearly circular, then the centre of motion will be situated in the plane passing through the sun perpendicular to the direction of his motion, and consequently in or near the great circle which has the point Q for one of its poles. The constellations through which this great circle passes are Piscis Australis, Pegasus, Andromeda, Perseus, &c. Argelander, from various considerations, thought it probable that the sun's orbit is nearly in the plane of the Milky Way, and therefore that the central body must be sought for in this plane also. Now the two points of the sphere in which the great circle which is 90° from Q (as above determined) intersects the plane of the Milky Way, are situated, the one in Perseus, Æ$=49°$, Dec. $=+54\frac{1}{2}°$, and the other and diametrically opposite one between Lupus and the Southern Triangle. Near one of these two points, therefore, the central point of the sun's orbit must be situated, if both suppositions are correct; and Argelander considers it most probable that the central point or body is in Perseus. Mädler, in a recent remarkable speculation, comes to the conclusion that the central sun is most probably situated in the Pleiades, and nearly in the direction of the star Alcyone (η Tauri) of that group. Perhaps the research is at present premature; but it seems not unreasonable to expect that a comparison of catalogues at the end of another half century will give the means of answering many interesting questions connected with the proper motions of the stars for the determination of which the data are still insufficient. It may then be possible to determine, for example, whether the apparent proper motions are uniform, or variable as has been supposed by Pond and Bessel; whether the direction of the sun's proper motion is gradually changing, or the apex maintains a fixed position in the heavens; whether the stars, which appear so irregularly grouped, form different independent systems, each having its own centre of attraction, or all obey the influence of one controlling force which pervades the visible universe. The solution of all these questions will, no doubt, be ultimately arrived at, but much yet remains to be done by the practical astronomer. Our knowledge of the proper motions of the southern stars is still very defective; and unless some other means are adopted than those which have yet been had recourse to, namely, the comparison of absolute places at

distant epochs, a long time must elapse before the deficiency is supplied, and we may still say, in the words of HALLEY, that centuries may be required to discover the laws of the proper motions of the stars.

Method of Calculation.

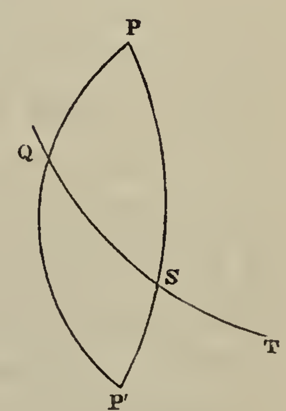
The direction of the apparent motion of a star is conveniently defined by the angle which it makes with the circle of declination. Let S be the place of the star found by reducing the place given in the first catalogue to the epoch of the second, S′ its place given in the second catalogue, and P the north pole of the equator. Connecting these points by arcs of great circles, the arc SS′ represents the proper motion of the star in the interval between the epochs, and the angle PSS′ is the angle which has been denoted by ψ. This angle, which gives the direction of the star's motion, is reckoned from left to right all round the circle, from $\psi=0$ to $\psi=360°$, and is computed from the variations of right ascension and declination indicated by the comparison of the catalogues as follows :—

Let α and δ denote respectively the right ascension and declination of the star at the mean epoch (1790), and $\Delta\alpha$, $\Delta\delta$ be the annual variations of those quantities arising from proper motion ($\Delta\alpha$ being in seconds of arc), and Δs the annual variation of the star's place in the arc of a great circle, we have then

$$\left.\begin{aligned} \Delta s \sin\psi &= \cos\delta\Delta\alpha \\ \Delta s \cos\psi &= \Delta\delta \\ \tan\psi &= \frac{\cos\delta\Delta\alpha}{\Delta\delta} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (1.)$$

The values of ψ and Δs calculated from these formulæ are given, for all the stars under consideration, in the appended table.

To determine the direction of the parallactic motion, let Q be the point towards which the sun's motion is assumed to be directed, T the point diametrically opposite, S the place of a star, P, P′ the north and south poles of the equator respectively, and let PSP′, PQP′ and QST be great circles of the sphere. In consequence of the real motion of the sun towards Q, the star, as seen from the earth, will appear to move towards T, in the great circle QST, the position of which will be given in terms of the angle PST which it makes with the declination circle PSP′. Let the angle PST be denoted by ψ', and let A and D denote respectively the assumed right ascension and declination of Q.



The angle ψ' may be computed immediately from the formula

$$\cot\psi' = -\cos SP' \cot QP'S + \frac{\sin SP' \cot QP'}{\sin QP'S},$$

in which all the quantities are known, since SP′ is given in terms of δ, the star's declination, QP′ in terms of D, and the angle QPS represents the difference between α

and A, the right ascension of the star and of the point Q. It is more convenient, however, to compute the side QS (which is required in the subsequent calculations) in the first place, and to make use of it in computing ψ', because the same logarithms which are required in proceeding after this manner serve also for computing the coefficients of the equations of condition.

The ordinary trigonometrical formulæ give

$$\cos QS = \cos QP' \cos SP' + \sin QP' \sin SP' \cos QP'S.$$

Now since Q is assumed to be on the north side of the equator, and all the stars included in the present investigation are on the south side, we have

$$\cos QP' = \cos (90° + D) = -\sin D; \quad \sin QP' = \sin (90° + D) = +\cos D.$$
$$\cos SP' = \cos (90° - \delta) = +\sin \delta; \quad \sin SP' = \sin (90° - \delta) = +\cos \delta; \quad QP'S = \alpha - A.$$

Denoting QS by χ, and substituting these values in the above formula, we get

$$\cos \chi = -\sin D \sin \delta + \cos D \cos \delta \cos (\alpha - A). \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Having found χ, or QS, the angle ψ' is computed from the formula

$$\sin \psi' = \frac{\sin QP' \sin QP'S}{\sin QS} = \frac{\cos D \sin (\alpha - A)}{\sin \chi}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

This sine belongs to two angles. In general there will be no difficulty with respect to the quadrant to which it belongs; but in a case of ambiguity, which may occur when ψ' is near 90°, recourse may be had to the formula for cot ψ' given above, which, on substituting for QP', SP', and QSP' their expressions in terms of D, δ, and $(\alpha - A)$, becomes

$$-\cot \psi' = +\sin \delta \cot (\alpha - A) + \frac{\tan D \cos \delta}{\sin (\alpha - A)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

In computing from the above formulæ attention must be paid to the changes of sign of $\cos (\alpha - A)$ and $\sin (\alpha - A)$. The most convenient mode of proceeding, perhaps, is to take the stars in the order of right ascension, beginning at the declination circle, passing through Q, and adding 360° to all the values of α which are less than the assumed value of A, that is, to the right ascensions of all the stars excepting those which lie between the declination circle which passes through Q and that which passes through the first point of Aries. The values of $(\alpha - A)$ will thus be expressed in a series proceeding from 0° to 360°, and the sign to be prefixed to the cosine or sine becomes known from the value of the angle.

The equations of condition are formed as follows:—

Differentiating equation (4.) on the supposition that A and D are the variable quantities, we get

$$\frac{d\psi'}{\sin^2 \psi'} = \frac{1}{\sin (\alpha - A)} \left\{ \sin \delta + \tan D \cos \delta \cos (\alpha - A) \right\} dA + \frac{\cos \delta}{\sin (\alpha - A) \cos^2 D} dD;$$

now $$\frac{\sin \psi'}{\sin (\alpha - A)} = \frac{\cos D}{\sin \chi}, \text{ therefore}$$

$$d\psi' = \frac{\cos D}{\sin^2 \chi} \left\{ \cos D \sin \delta + \sin D \cos \delta \cos (\alpha - A) \right\} dA + \frac{\cos \delta \sin (\alpha - A)}{\sin^2 \chi} dD. \quad . \quad . \quad (5.)$$

If we now substitute for $d\psi'$ the difference between the angles ψ and ψ', or the value of $\psi-\psi'$ as found by equations (1.) and (3.), we shall have an equation in which dA and dD are the only unknown quantities. Every star furnishes a similar equation; and the values of dA and dD deduced from the whole of the equations by the method of least squares give the corrections to be applied to A and D, the assumed right ascension and declination of Q. Before this method can be applied, however, it is necessary to consider how the observations are affected by the situation and other circumstances of the individual stars, in order that all the equations may be reduced to the same degree of precision.

In the present inquiry it is assumed that the positions given in the catalogues, and the reductions from the first epoch to the second, are equally precise for all the stars; and in respect of the true proper motion, it has already been stated that we are not possessed of data to enable us to make any distinction between one star and another, and must therefore assume that, in this respect, all the equations have the same weight. Confining our consideration, therefore, to that part of the apparent motion which is caused by the displacement of the sun, the relative weights of the equations are determined as follows:—

Let the parallactic motions in right ascension and declination, in the unit of time (here assumed to be one year), be denoted by $\Delta\alpha$ and $\Delta\delta$ respectively, and the corresponding motion in the arc of a great circle by Δs, and we have the equations

$$\Delta s \sin\psi' = \cos\delta\Delta\alpha\,;\quad \Delta s\cos\psi' = \Delta\delta,$$

by differentiating which we get

$$d\Delta s\sin\psi' + \Delta s\cos\psi' d\psi' = d(\cos\delta\Delta\alpha),$$

$$d\Delta s\cos\psi' - \Delta s\sin\psi' d\psi' = d\Delta\delta,$$

whence

$$\Delta s d\psi' = \cos\psi' d(\cos\delta\Delta\alpha) - \sin\psi' d\Delta\delta.$$

Denoting in general the probable error of any quantity x by $\varepsilon(x)$, and its square by $\varepsilon^2(x)$, and observing that if $x=y\pm z$ the theory of probable errors gives

$$\varepsilon(x) = \sqrt{\{\varepsilon^2(y)+\varepsilon^2(z)\}},$$

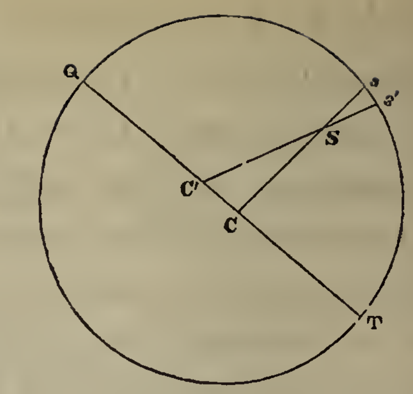
we have in respect of the above equation

$$\Delta s\,\varepsilon(\psi') = \sqrt{\{\cos^2\psi'\varepsilon^2(\cos\delta\Delta\alpha) + \sin^2\psi'\varepsilon^2(\Delta\delta)\}}.$$

Now the probable errors of $\Delta\alpha$ and $\Delta\delta$ depend manifestly on the precision and number of the observations from which the places of the stars have been determined; and as minute accuracy is not attainable in the present case, it may be assumed that the places of all the stars in each catalogue have been determined with equal precision. It may also be assumed that in respect of an equatorial star the probable error in right ascension is equal to the probable error in declination, and, generally, that $\varepsilon(\cos\delta\Delta\alpha)=\varepsilon(\Delta\delta)$. Denoting therefore the constant error by e, these two assumptions give $\varepsilon(\cos\delta\Delta\alpha)=\varepsilon(\Delta\delta)=e$, and the above equation becomes

$$\Delta s\,\varepsilon(\psi') = e\,;$$

whence it appears that the probable error of ψ' is inversely proportional to Δs.



To determine this quantity, let C be the place of the sun at the beginning, and C′ its place at the end of the time t, S the place of the star, and let straight lines joining CS and C′S meet the great circle whose plane contains the points C, C′, S, in s and s'. This plane also contains the points Q and T, which, therefore, are in the circumference of the same great circle. Now the angle CSC′, being the difference between QCS and QC′S, is the parallax, or angular variation of the apparent place of the star, which in consequence of the motion of the sun from C to C′ will appear to have moved from s to s', and is therefore (making $t=$ one year) the angle denoted by Δs. Hence the triangle CC′S gives

$$\sin \Delta s = \frac{\text{CC}' \times \sin \text{QC}'\text{S}}{\text{CS}}.$$

Let CC′, which is constant, be denoted by R, and CS, the distance of the star, by r; then, observing that QC′S the angular distance of the star's place from Q is the angle which has been denoted by χ, and that $\sin \Delta s = \Delta s \sin 1''$, the above equation becomes

$$\Delta s = \frac{\text{R} \sin \chi}{r \sin 1''}.$$

Substituting this in the equation $\Delta s\, \varepsilon(\psi') = e$, we have

$$\frac{\text{R} \sin \chi}{r \sin 1''} \varepsilon(\psi') = e; \text{ or } \frac{\sin \chi}{r} \varepsilon(\psi') = \text{ a constant};$$

whence it follows that in order to reduce all the equations of condition to the same degree of precision, it is necessary to multiply each by $\sin \chi$, and to divide by a number proportional to r. The relative distances of the stars are however unknown, and in the present inquiry it has been assumed that they are all at the same mean distance, that is, r is assumed to be constant; and accordingly $\sin \chi$ becomes the measure of the precision of the equation.

Multiplying equation (5.) by $\sin \chi$, the formula for the equations of condition becomes

$$\sin \chi d\psi' = \frac{\cos \text{D}}{\sin \chi} \left\{ \cos \text{D} \sin \delta + \sin \text{D} \cos \delta \cos (\alpha - \text{A}) \right\} d\text{A} + \frac{\cos \delta \sin (\alpha - \text{A})}{\sin \chi} d\text{D}; \quad . \quad . \quad (6.)$$

or, since
$$\frac{\cos \text{D}}{\sin \chi} = \frac{\sin \psi'}{\sin (\alpha - \text{A})},$$

$$\sin \chi d\psi' = \frac{\sin \psi}{\sin (\alpha - \text{A})} \left\{ \cos \text{D} \sin \delta + \sin \text{D} \cos \delta \cos (\alpha - \text{A}) \right\} d\text{A} + \frac{\cos \delta \sin \psi'}{\cos \text{D}} d\text{D}. \quad . \quad . \quad (7.)$$

The double equation affords some advantage in checking the calculations; and it will be observed that the logarithms of all the sines and cosines required for computing the coefficients of dA and dD have already been used for computing χ and ψ'. It may be proper to state, that although in the appended table the logarithms of the sines and cosines of the different angles, and the coefficients of dA and dD in the

equations of conditions, are given to four decimal places, all the calculations were made from the tables of five-figure logarithms.

The weights and probable errors of the values of dA and dD deduced from the equations of condition by the method of least squares, and the probable error of Ψ, or probable value of $(\psi-\psi')\sin\chi$, are computed according to the following formulæ:

Putting the above equation (7.) under the form

$$0=ad\mathrm{A}+bd\mathrm{D}-n,$$

and assuming, according to the usual notation, (aa) to denote the sum of the squares of the coefficient a, (bb) the sum of the squares of b, (nn) the sum of the squares of n, (ab) the sum of the products ab, and so on, and denoting also the *weights* of dA and dD respectively by $w(d\mathrm{A})$, $w(d\mathrm{D})$, the formulæ of the method of least squares give

$$d\mathrm{A}=-\frac{(bb)(an)-(ab)(bn)}{(aa)(bb)-(ab)(ab)};\quad d\mathrm{D}=-\frac{(aa)(bn)-(ab)(an)}{(aa)(bb)-(ab)(ab)}$$

$$w(d\mathrm{A})=\frac{(aa)(bb)-(ab)(ab)}{(bb)};\quad w(d\mathrm{D})=\frac{(aa)(bb)-(ab)(ab)}{(aa)}.$$

Let h denote the number of equations, $\varepsilon(\Psi)$ the probable error of Ψ, $\varepsilon(d\mathrm{A})$ and $\varepsilon(d\mathrm{D})$ the probable errors of dA and dD, and assume

$$(nn.2)=(nn)-\frac{(an)^2}{(aa)}-\frac{(bn.1)^2}{(bb.1)},$$

where

$$(bn.1)=(bn)-\frac{(ab)}{(aa)}(an);\quad (bb.1)=(bb)-\frac{(ab)}{(aa)}(ab),$$

then the theory gives

$$\varepsilon(\Psi)=\cdot 67449\sqrt{\frac{(nn.2)}{h-2}};\qquad \varepsilon(d\mathrm{A})=\frac{\varepsilon(\Psi)}{\sqrt{w(d\mathrm{A})}};\qquad \varepsilon(d\mathrm{D})=\frac{\varepsilon(\Psi)}{\sqrt{w(d\mathrm{D})}}.$$

It may be remarked that in these formulæ $(nn.2)$ denotes the sum of the squares of the remaining errors when the values of dA and dD, found as above, are substituted in the equations of condition.

In the following table (which has been frequently referred to) the names of the stars are given, with their mean places for 1790, and the principal details of the calculation. The columns headed "LACAILLE — JOHNSON," "LACAILLE — HENDERSON," "BRADLEY — HENDERSON," contain the proper motions as given by Mr. JOHNSON and Prof. HENDERSON in their respective catalogues for the whole interval between the compared catalogues, those in Æ being in seconds of time. In the column of difference in Æ the positive sign shows that the right ascension is greater in the ancient catalogue than in the modern, and has consequently been diminished, and the negative that it has been increased, through the effect of the proper motion. In the column of difference in declination, the positive sign indicates a proper motion towards the south, and the negative a proper motion towards the north; the stars being all in the southern hemisphere, and their declinations consequently supposed to be affected with the negative sign.

No.	Star's name.	Magnitude.	α (1790).	δ (1790).	Lacaille — Johnson. Æ.	Dec.	Lacaille — Henderson. Æ.	Dec.
			° ′	° ′	s	″	s	″
1	η Pavonis	4·5	261 17·6	64 35·5	+ 0·24	+14·0	....	....
2	ι^1 Scorpii	3	263 13·8	40 1·4	+ 0·24	+ 6·6	+ 0·57	+ 6·1
3	β Telescopii	4	270 51·3	36 48·3	+ 0·86	+14·6	....	....
4	ε Sagittarii	3	272 33·6	34 27·8	....	....	+ 0·79	+15·4
5	ζ Pavonis	4	274 36·5	71 34·4	+ 0·28	+14·4	....	....
6	β^1 Sagittarii	3·4	286 52·6	44 49·9	— 0·36	+ 9·6	....	....
7	α Sagittarii	4·5	287 19·6	40 59·5	— 0·64	+12·4	....	....
8	ε Pavonis	4	293 59·8	73 26·1	— 1·35	+ 8·3	....	....
9	δ Pavonis	4	296 59·3	66 41·5	—16·28	+92·4	....	....
10	α Pavonis	2	302 13·8	57 23·4	— 0·79	+ 4·7	....	....
11	α Indi	3	305 40·8	48 0·4	— 0·92	— 3·8	....	....
12	γ Pavonis	3	317 12·6	66 17·9	— 1·21	—60·6	— 1·08	—60·7
13	γ Gruis	3	325 17·4	38 20·6	— 0·81	+ 7·1	....	....
14	α Gruis	2	328 43·7	47 58·2	— 1·26	+17·2	— 1·07	+15·3
15	β Octantis	5	335 50·2	82 28·3	+ 5·85	+ 0·7	+ 7·23	+ 0·6
16	β Gruis	3	337 30·6	47 58·5	— 1·65	+ 4·1	— 1·54	+ 4·6
17	α Piscis Aust.	1	341 30·2	30 43·8	....	....	— 1·93	+16·6
18	γ^1 Octantis	5	354 48·1	83 11·1	+10·51	+ 0·3	+11·62	+ 0·83
19	Toucan	5	2 15·4	66 6·6	—20·95	—95·9	....	....
20	β Hydri	3	3 36·9	78 26·3	—57·38	—23·1	—59·41	—24·3
21	α Phœnicis	2	3 58·0	43 26·9	— 1·33	+36·4	— 1·15	+36·8
22	η Phœnicis	5	8 28·0	58 36·7	+ 1·10	+42·3	....	....
23	γ Phœnicis	3·4	19 48·4	44 23·9	+ 0·09	+20·9	+ 0·33	+20·6
24	δ Phœnicis	4	20 37·3	50 10·1	— 1·23	— 9·6	....	....
25	χ Eridani	4	26 56·8	52 39·6	— 4·84	—25·2	....	....
26	α Hydri	3·4	28 2·3	62 35·8	— 3·00	— 1·6	— 2·75	— 1·4
27	β Reticuli	4	55 24·5	65 28·3	— 2·43	—12·0	....	....
28	52 Eridani	3	66 51·0	31 0·2	....	....	+ 0·69	+ 5·1
29	β Columbæ	3	85 53·1	35 51·5	+ 0·04	—32·7	+ 0·33	—31·0
30	α Equ. Pict.	4	101 30·5	61 43·1	+ 1·65	—24·4	....	....
31	σ Argûs	4	110 38·7	42 53·1	+ 0·77	—10·6	....	....
32	γ^2 Argûs	2	120 46·0	46 43·5	+ 0·63	+ 6·0	+ 0·86	+ 5·0
33	ε Argûs	2	124 32·9	58 50·4	+ 0·03	— 7·3	+ 0·31	— 8·4
34	δ Argûs	3	129 43·7	53 56·7	+ 0·21	+12·6	+ 0·39	+11·9
35	α Pisc. Vol.	4·5	134 46·5	65 33·7	— 0·65	+10·4	....	....
36	G in C Argûs	5	136 7·4	71 45·1	+ 1·76	+43·5	....	....
37	β Argûs	1·2	137 42·5	68 51·3	+ 2·61	— 7·0	+ 2·53	— 8·7
38	δ^2 Chamæl.	5	160 54·8	79 26·0	+ 3·77	— 0·8	....	....
39	ε Chamæl.	5	177 22·0	77 3·1	+ 3·24	+ 5·1	....	....
40	δ Centauri	3	179 23·3	49 33·1	+ 0·25	+ 7·9	+ 0·60	+ 7·3
41	δ Crucis	3	181 1·6	57 34·9	+ 0·60	— 2·0	+ 0·99	— 3·5
42	β Chamæl.	5	181 36·4	78 8·7	+ 3·24	+ 3·9	+ 3·79	+ 1·5
43	ε Crucis	4	182 32·2	59 14·4	+ 2·01	—13·7	....	....
44	α^1 Crucis	1	183 46·1	61 56·0	+ 1·64	— 5·9	+ 1·68	— 5·0
45	γ Crucis	2·3	184 54·5	55 56·2	— 0·56	+15·3	— 0·19	+15·7
46	γ Muscæ	4	185 2·6	70 58·3	+ 1·36	+ 5·3	....	....
47	γ Centauri	3	187 30·3	47 48·2	+ 1·56	+ 6·6	+ 1·96	+ 5·1
48	ι Centauri	3	197 12·8	35 36·0	+ 1·85	+ 8·4	+ 2·33	+ 9·4
49	ε Centauri	3	201 40·6	52 23·4	+ 0·58	+ 8·6	+ 1·03	+ 7·6
50	3 Centauri	4·5	204 56·5	31 56·7	+ 0·83	+ 2·1	....	....
51	β Centauri	1	207 17·7	59 21·0	+ 0·51	+ 5·7	+ 0·95	+ 5·5
52	θ Centauri	3	208 35·9	35 19·7	+ 3·42	+45·7	+ 3·87	+45·1
53	δ Octantis	5	208 50·3	82 40·9	+ 8·00	+ 2·5	+ 9·12	+ 1·2
54	ι Lupi	4·5	211 30·7	45 4·7	— 0·96	+ 5·6	....	....
55	α^1 Centauri	4	216 21·8	59 57·6	+37·21	—65·3	+38·44	—68·5
56	α Lupi	3	217 0·9	46 28·4	+ 0·16	+ 8·2	+ 0·63	+ 6·5
57	β Lupi	3	221 12·9	42 16·4	— 0·04	+12·6	+ 0·42	+11·5
58	π Lupi	5	222 43·7	46 12·8	+ 0·84	+ 7·8	....	....
59	γ Triang. Aust.	3	224 23·9	67 53·0	+ 1·33	+ 2·8	+ 2·03	+ 3·0
60	β Triang. Aust.	3	234 12·2	62 45·6	+ 1·88	+33·4	+ 2·45	+32·8
61	ε Scorpii	3	249 8·9	33 53·6	....	....	+ 4·15	+23·8
62	η Scorpii	3·4	254 17·2	42 56·4	— 0·28	+25·1	— 0·04	+25·0
63	β Aræ	3	256 58·3	55 18·4	— 0·25	+ 9·4	....	....
64	δ Aræ	4	258 3·0	60 28·8	+ 0·44	+ 8·5	....	....
65	α Aræ	3	258 54·5	49 41·1	— 0·24	+ 9·0	....	....

No.	Log $\Delta\alpha$.	Log $\Delta\delta$.	Δs.	ψ.	ψ'.	$\psi - \psi'$.	Log sin χ.
			″	° ′	° ′	° ′	
1	8·6532	9·2430	0·176	186 17·8	178 42·2	+ 7 35·6	9·9967
2	8·8692	8·8921	0·096	216 0·0	176 56·5	+ 39 3·5	9·9796
3	9·2075	9·2613	0·224	215 16·7	170 4·0	+ 45 12·7	9·9732
4	9·1546	9·2684	0·220	212 23·5	168 23·1	+ 44 0·4	9·9673
5	8·7202	9·2553	0·181	185 16·0	167 6·2	+ 18 9·8	9·9858
6	8·8293n	9·0792	0·129	158 15·2	157 6·7	+ 1 8·5	9·9948
7	9·0792n	9·1903	0·180	149 42·0	156 28·0	− 6 46·0	9·9900
8	9·4033n	9·0160	0·127	145 7·0	150 0·0	− 4 53·0	9·9772
9	0·4847n	0·0626	1·671	133 43·2	148 24·3	− 14 41·1	9·9885
10	9·1706n	8·7690	0·099	126 21·1	144 59·3	− 18 38·2	9·9967
11	9·2368n	8·6767n	0·125	67 37·8	142 42·7	− 75 4·9	0·0000
12	9·3243n	9·8718n	0·749	6 30·1	131 37·5	−125 7·4	9·9782
13	9·1815n	8·9482	0·149	126 41·2	129 44·4	− 3 3·2	9·9992
14	9·3321n	9·3003	0·246	144 14·1	126 36·0	+ 17 38·1	9·9915
15	0·1167	7·9026	0·172	267 19·8	108 21·8	+158 58·0	9·9358
16	9·4681n	8·7271	0·204	105 10·3	120 52·2	− 15 41·9	9·9824
17	9·5426n	9·3010	0·360	123 42·4	122 6·6	+ 1 35·8	9·9936
18	0·3086	7·3757	0·242	268 33·3	89 20·3	+179 13·0	9·9244
19	0·5942n	0·0787n	1·992	53 0·0	93 32·0	− 40 32·0	9·9165
20	1·0313n	9·4635n	2·175	82 18·8	83 43·5	− 1 24·7	9·9159
21	9·3591n	9·6524	0·479	159 43·3	106 51·2	+ 52 52·1	9·9317
22	9·3144	9·7233	0·540	191 29·0	93 30·0	+ 97 59·0	9·9033
23	8·5827	9·4061	0·256	186 7·5	96 54·0	+ 89 13·5	9·8666
24	9·3629n	9·0792n	0·190	50 54 7	90 10·0	− 39 15·3	9·8552
25	9·9579n	9·4983n	0·634	60 13·1	83 16·0	− 23 2·9	9·8304
26	9·7241n	8·2655n	0·244	85 40·6	71 54·0	+ 13 46·6	9·8431
27	9·6586n	9·1761n	0·241	51 35·1	36 10·0	+ 15 25·1	9·7705
28	9·0959	8·7885	0·123	240 6·3	101 11 0	+138 55·3	9·2838
29	8·5260	9·5923n	0·392	356 1·3	301 52·9	+ 54 8·4	9·0240
30	9·4905	9·4843n	0·338	334 19·9	324 20·3	+ 9 59·6	9·7291
31	9·1595	9·1222n	0·170	321 23·8	282 53·0	+ 38 30·8	9·6474
32	9·1360	8·8301	0·116	234 12·0	279 47·5	− 45 35·5	9·7493
33	8·4891	8·9833n	0·098	350 35·0	291 31·4	+ 59 3·6	9·8053
34	8·7398	9·1774	0·154	192 7·7	280 44·5	− 88 36·8	9·8177
35	9·0859n	9·1139	0·139	158 48·0	287 31·2	−128 43·2	9·8601
36	9·5185	9·7354	0·554	190 45·6	291 25·9	−100 40·3	9·8775
37	9·6751	8·9830n	0·196	299 23·3	287 23·7	+ 11 59·6	9·8745
38	9·8493	8·0000n	0·130	274 24·7	272 1·0	+ 2 23·7	9·9211
39	9·7836	8·8045	0·150	244 54·3	254 40·7	− 9 46·4	9·9380
40	8·8901	8·9701	0·106	208 21·2	241 16·6	− 32 55·4	9·9769
41	9·1635	8·5261n	0·085	293 15·7	242 41·9	+ 50 33·8	9·9689
42	9·8104	8·5239	0·137	255 52·5	251 17·0	+ 4 35·5	9·9403
43	9·5762	9·2336n	0·258	311 37·2	242 8·0	+ 69 29·2	9·9687
44	9·4851	8·8260n	0·159	294 59·0	242 7·5	+ 52 51·5	9·9665
45	8·8431n	9·2792	0·194	168 24·4	239 18·1	− 70 53·7	9·9763
46	9·4065	8·8212	0·106	231 27·0	244 40·0	− 13 13·0	9·9544
47	9·5097	8·8572	0·229	251 40·0	235 33·0	+ 16 7·0	9·9887
48	9·5847	9·0379	0·331	250 44·0	228 27·8	+ 22 16·2	0·0000
49	9·1687	8·9980	0·134	222 7·0	226 26·2	− 4 19·2	9·9948
50	9·1921	8·4191	0·135	258 45·5	224 2·5	+ 34 43·0	9·9964
51	9·1258	8·8373	0·097	224 44·5	223 3·5	+ 1 41·0	9 9911
52	9·8263	9·7461	0·781	224 27·5	221 26·7	+ 3 0·8	9·9968
53	0·1970	8·3591	0·202	263 29·7	227 42·5	+ 35 47·2	9·9471
54	9·2553n	8·8451	0·145	118 50·6	219 0·5	−100 9·9	9·9999
55	0·8427	9·9142n	3·581	283 15·1	215 56·8	+ 67 18·3	9·9945
56	8·8569	8·9562	0·103	208 43·3	214 57·5	− 6 14·2	9·9997
57	8·5340	9·1703	0·150	189 42·1	212 0·2	− 22 18·1	9·9965
58	9·1973	8·9890	0·146	228 11·1	210 38·5	+ 17 32·6	9·9987
59	9·4887	8·5511	0·121	252 57·2	210 10·2	+ 42 47·0	9·9875
60	9·5995	9·6089	0·445	204 6·7	201 32·2	+ 2 34·5	9·9963
61	9·8751	9·4575	0·686	245 16·2	189 42·8	+ 55 33·4	9·9645
62	8·4752n	9·4878	0·308	175 55·9	184 46·4	− 8 50·5	9·9861
63	8·6709n	9·0700	0·121	167 12·4	182 21·7	− 15 9·3	9·9997
64	8·9165	9·0263	0·114	200 56·2	181 27·2	+ 19 29·0	9·9994
65	8·6532n	9·0512	0·116	165 29·4	180 44·0	− 15 14·6	9·9960

No.	Star's name.	α (1790).	δ (1790).	Bradley — Henderson. Æ.	Dec.
		° ′	° ′	s	″
66	β Ceti	8 15·6	19 8·5	−1·10	− 1·5
67	θ[1] Ceti	18 23·0	9 16·4	+0·41	+16·9
68	ζ Ceti	25 16·5	11 23·7	−0·05	+10·6
69	η Eridani	41 32·7	9 44·6	−0·47	+17·6
70	γ[1] Eridani	57 3·6	14 7·0	−0·37	+ 8·1
71	β Eridani	74 23·0	5 22·3	+0·46	+ 5·9
72	α Canis Maj.	98 58·3	16 26·6	+2·67	+97·2
73	15 Argûs	119 37·4	23 37·2	+0·41	− 5·9
74	δ Hydræ et Crat.	167 13·1	13 38·7	+0·53	−13·1
75	γ Corvi	181 15·8	16 22·6	+0·90	− 1·7
76	δ Corvi	184 45·2	15 20·7	+0·43	+12·1
77	β Corvi	185 51·0	22 19·0	+0·55	+ 5·7
78	α[1] Libræ	219 51·5	15 6·3	+0·51	+ 5·8
79	δ Ophiuchi	240 50·4	3 8·5	+0·32	+ 9·5
80	η Ophiuchi	254 36·6	15 27·0	−0·13	− 8·3
81	σ Sagittarii	281 31·3	26 32·5	−0·15	+ 7·6

No.	Log Δα.	Log Δδ.	Δs.	ψ.	ψ′.	ψ−ψ′.	Log sin χ.
			″	° ′	° ′	° ′	
66	9·3254n	8·2840n	0·201	84 30·2	117 41·5	− 33 11·3	9·9559
67	8·8968	9·3358	0·230	199 45·3	122 8·1	+ 77 37·2	9·9417
68	7·9830n	9·1332	0·136	176 2·0	122 2·9	+ 53 59·1	9·9085
69	8·9561n	9·3534	0·243	158 27·4	128 39·0	+ 29 48·4	9·8249
70	8·8522n	9·0164	0·125	146 23·8	135 28·6	+ 10 55·2	9·6669
71	8·9468	8·8788	0·116	229 20·6	170 9·2	+ 59 11·4	9·6656
72	9·7105	0·0956	1·340	201 33·8	223 45·1	− 22 11·3	9·6033
73	8·8968	8·8788n	0·105	316 19·0	246 42·5	+ 69 36·5	9·7697
74	9·0083	9·2252n	0·195	329 28·2	238 39·4	+ 90 48·8	9·9941
75	9·2382	8·3384n	0·167	277 28·6	235 45·0	+ 41 43·6	0·0000
76	8·9175	9·1907	0·174	207 12·4	234 45·2	− 27 32·8	9·9990
77	9·0244	8·8638	0·122	233 14·8	234 8·8	− 0 54·0	0·0000
78	8·9916	8·8713	0·120	231 15·5	218 13·1	+ 13 2·4	9·9419
79	8·7892	9·0856	0·136	206 46·3	205 15·2	+ 1 31·1	9·8071
80	8·3979n	9·0270n	0·109	12 45·6	185 50·5	−173 4·9	9·8724
81	8·4601n	8·9887	0·101	165 10·0	160 15·9	+ 4 54·1	9·9470

Equations of Condition.

From comparison with Lacaille's Catalogue.

1	0=+·8434dA	+·0115dD	− 7·54
2	0=+·8425	+·0484	− 37·27
3	0=+·8321	+·1638	− 42·50
4	0=+·8269	+·1968	− 40·81
5	0=+·8405	+·0837	− 17·58
6	0=+·7971	+·3270	− 1·13
7	0=+·7878	+·3573	+ 6·61
8	0=+·8313	+·1690	+ 4·63
9	0=+·8176	+·2458	+ 14·30
10	0=+·7868	+·3666	+ 18·50
11	0=+·7397	+·4805	+ 75·08
12	0=+·7880	+·3563	+119·00
13	0=+·5896	+·7151	+ 3·05
14	0=+·6500	+·6373	− 17·29
15	0=+·8342	+·1474	−137·12
16	0=+·6174	+·6813	+ 15·07
17	0=+·4257	+·8632	− 1·57
18	0=+·8351	+·1406	−150·79
19	0=+·7403	+·4792	+ 33·44
20	0=+·8196	+·2362	+ 1·16
21	0=+·4782	+·8238	− 45·20
22	0=+·6642	+·6164	− 78·46
23	0=+·4564	+·8410	− 65·63
24	0=+·5488	+·7594	+ 28·12
25	0=+·5904	+·7141	+ 15·60
26	0=+·7210	+·5187	− 9·60
27	0=+·8071	+·2905	− 9·09
28	0=+·0630	−·9810	+ 26·71
29	0=+·4883	−·8169	− 5·72
30	0=+·7970	−·3275	− 5·36
31	0=+·4486	−·8469	− 17·10
32	0=+·5050	−·8009	+ 25·60
33	0=+·6926	−·5707	− 37·72
34	0=+·6140dA	−·6856dD	+ 58·24
35	0=+·7455	−·4678	+ 93·27
36	0=+·7921	−·3456	+ 75·94
37	0=+·7700	−·4081	− 8·98
38	0=+·8233	−·2173	− 2·00
39	0=+·8153	−·2562	+ 8·47
40	0=+·6227	−·6745	+ 31·22
41	0=+·6960	−·5648	− 47·07
42	0=+·8207	−·2307	− 4·00
43	0=+·7102	−·5361	− 64·66
44	0=+·7338	−·4391	− 48·94
45	0=+·6924	−·5710	+ 67·12
46	0=+·7903	−·3494	+ 11·90
47	0=+·6361	−·6567	− 15·70
48	0=+·5839	−·7216	− 22·27
49	0=+·7182	−·5243	+ 4·27
50	0=+·6028	−·6994	− 34·43
51	0=+·7683	−·4127	− 1·65
52	0=+·6479	−·6403	− 2·99
53	0=+·8381	−·1117	− 3·68
54	0=+·7169	−·5270	+100·15
55	0=+·7906	−·3484	− 66·46
56	0=+·7433	−·4677	+ 6·23
57	0=+·7467	−·4649	+ 22·12
58	0=+·7662	−·4181	− 17·49
59	0=+·8226	−·2243	− 41·57
60	0=+·8266	−·1992	− 2·53
61	0=+·8317	−·1661	− 51·19
62	0=+·8412	−·0722	+ 8·55
63	0=+·8431	−·0278	+ 15·14
64	0=+·8434	−·0148	− 19·46
65	0=+·8434	−·0098	+ 15·10

From comparison with Bradley's Catalogue.

66	0=+·1080dA	+·9918dD	+ 29·98
67	0=+·1138	−·9908	+ 67·87
68	0=+·1449	−·9851	+ 43·73
69	0=+·3448	−·9125	+ 19·92
70	0=+·4992	−·8062	+ 5·07
71	0=+·8264	−·2019	+ 27·41
72	0=+·5211	+·7864	− 8·90
73	0=+·0572	+·9978	+ 40·96
74	0=+·1502dA	−·9840dD	− 89·60
75	0=+·2872	−·9404	− 41·72
76	0=+·3020	−·9338	+ 27·48
77	0=+·3863	−·8890	+ 0·90
78	4=+·5955	−·7082	− 11·41
79	0=+·7280	−·5050	− 0·97
80	0=+·8377	−·1163	+129·57
81	0=+·7875	+·3581	− 4·34

X. *On Photographic Self-registering Meteorological and Magnetical Instruments.*

By Francis Ronalds, *Esq., F.R.S.*

Received November 18, 1846,—Read January 21, 1847.

The Council of the Royal Society having, in April last, granted to me, out of the Donation Fund, fifty pounds "for the purchase of magnetical and meteorological instruments" necessary to the prosecution of experiments in which I was then (and had been long previously) engaged, I feel very anxious to express, now, my deep sense of obligation for its munificence, and to endeavour to show that successful and satisfactory results, in great measure due to the timely assistance thus kindly afforded, have been attained.

It would be superfluous to speak of those proposals of other gentlemen, or of my own, for self-registering, photographically, the variations of the declination magnet and the thermometer which were made previously to *the use of good achromatic lenses, for projecting, upon photographic paper, a sharp image, magnified to any required degree, of that part of the instrument whose motions are to be registered.*

This is the principal feature of the system which I have hitherto applied to procure the self-registration of the atmospheric electrometer, the thermometer, the barometer, and the declination magnetometer, and which I propose to apply to every other meteorological and terrestro-magnetical instrument. Although it had long occupied my thoughts, had received some approbation from the Astronomer Royal in April 1845, and had been the object in view in some experiments made in conjunction with Mr. H. Collen of Somerset Street, on photographic paper in the beginning of July 1845, yet my other contrivances and occupations at the Kew Observatory (elsewhere detailed) prevented the completion of any apparatus for actual registration until the end of that month.

The photo-electrograph as then constructed, and since improved, may be thus described.

A rectangular box, about 16 inches long and 3 inches square, constitutes the part usually called the "body" of a kind of lucernal microscope. A voltaic electrometer (properly insulated, and in communication with an atmospheric conductor) is suspended within this microscope, through an aperture in the upper side, and near to the *object* end. That end itself is closed by a plane of glass, when daylight is used, and by condensing lenses, when a common Argand lamp is employed. In either case an abundance of light is thrown into the microscope. Between the electrometer and the other, or *eye-end* of the microscope, fine achromatic lenses are placed, which have the double effect of condensing the light upon a small screen, situated at that eye-end, and of

projecting a strong image of the electrometer, in deep *oscuro*, upon it. Through the screen a very narrow slit, of proper curvature, is cut (the chord of the arc being in a horizontal position), and it is fitted into the back of a case, about two and a half feet long, which case is fixed to the eye-end of the microscope, at right angles with its axis, and vertically. Within this case is suspended a frame, provided with grooves, into which two plates of pure thin glass can be dropped, and brought into close contact by means of six little bolts and nuts. This frame can be removed at pleasure from a line, by which it is suspended, and the line after passing through a small aperture (stopped with grease) cut through the upper end of the long case, is attached to a pulley (about 4 inches in diameter) fixed, with capacity of adjustment, on the hour-arbor of a good clock. Lastly, counterpoises, rollers, springs, and a straight ruler are employed, for ensuring accurate rectilineal sliding of the frame when the clock is set in motion.

A piece of properly prepared photographic paper is now placed between the two plates of glass in the moveable frame; the frame is removed (in a box made purposely, for excluding light) and is suspended in the long case; this is closed so as to prevent the possibility of extraneous light entering it; the clock is started; and the time of starting is noted.

All that part of the paper which is made to pass over the slit in the screen, by the motion of the clock, becomes now therefore successively exposed to a strong light; and is consequently brought into a state which fits it to receive a dark colour on being again washed with the usual solutions, *excepting* those small portions upon which dark images of the lower parts of the pendulums of the electrometer are projected through the slit; these small portions of course retain the light colour of the paper; and form the long curved lines or bands, whose distances from each other at any given part of the photograph (*i. e.*), at any given time, indicate the electric tension of the atmosphere at that time.

Sometimes, when daylight was used, various appearances of the sky were noted during the process, by which it would seem, that, in serene weather, when the sun's light and heat varied, and the paper became consequently more or less darkened, the electric tension, as shown in the photograph, varied also; increasing with the increment of light, &c. This fact has not perhaps been before observed: and some attentive observations on the subject, made with the aid of a *good* actinometer, &c., are desirable.

In order that the state of the electrometer itself may be known at any period of the process, a small microscope is fitted to an aperture in the door of the long case, opposite to the slit in the screen, and arrangements are made whereby the eye may be applied to it, and to view the images through the semitransparent photographic paper, without damage by the admission of extraneous light.

The adjustment of the lenses in the body of the lucernal microscope, for procuring the best possible *chemical* focus, can only be obtained by a short series of experiments; but having been once found, future adjustments are not necessary.

In pursuance of a hint or suggestion of the Astronomer Royal, a very useful addition to the electrometer has been made, by which *the kind* in addition to the *tension* of the electrical charge is registered. It depends upon the same principle as that of the "*Dry Pile* distinguishing apparatus," and will be clearly understood by reference to the diagram.

The above-described instrument, with its various improvements, made during the progress of the experiments, was used in connection with an atmospheric conductor situated much lower, and otherwise much less advantageously than our ordinary Kew-conductor*.

In order to adapt the greater part of the apparatus to the purposes of a *thermograph*, a thermometer of the horizontal kind, and having a flat bore, is employed. Its tube is introduced through a side of the microscope in lieu of the electrometer; a diaphragm is fixed upon it of narrower dimensions than the breadth of the mercury; and the slit in the screen at the eye-end of the microscope is (of course) straight and horizontal; the manipulation and all else remaining as before.

The photo-barometrograph requires a somewhat different arrangement of the same microscope, &c.

The long case in which the frame carrying the photographic paper slides, is now placed in a horizontal position. The clock with its pulley, &c. is fixed near to one end of that case: the usual line, attached to the pulley on the clock-arbor, enters the case, as before, and is hooked to the sliding frame, and provision is made of the same kind as before, &c. for ensuring its steady and regular motion.

The lower leg of a siphon barometer is introduced through the *now* lower side of the microscope (in lieu of the thermometric tube), and a very light blackened pith-ball rests upon the surface of the mercury. In order to procure a clean and correct boundary-line in the photograph, a kind of contracting and expanding diaphragm on the barometer tube became very useful in this case, for when daylight was used, very minute adjustments of the aperture were required. The slit in the screen is (of course) vertical. The curves seen in the photographs represent the actual variations nearly of this *siphon* barometer, but it is my intention to try to fill a barometer of the *cistern* kind, in the late Professor Daniell's manner, with a pith-ball on the surface of the mercury still, and then to use a magnified range of the image.

The curve or right line which forms the right-hand boundary of the dark band, seen in the photographs of the thermometer and barometer, represents the variations of the mercury as accurately as the usual scale readings of ordinary instruments.

The applicability of this system of self-registration to a *magnetograph* was sufficiently obvious; but a much more solid kind and disposition of apparatus is necessary.

The two-feet magnet, now used for this purpose at the Kew Observatory, was kindly lent to me by the Astronomer Royal (in February 1846), and is suspended by

* The photographs executed between July 24, 1845, and August 29, 1846, by means of this and my various other instruments, were produced at the meeting of the Royal Society, at which this communication was read.

a silken skein of the same kind as that used for suspending the Greenwich declination magnet. The damper surrounding the magnet is of mahogany coated (by means of the electrotype process) with pure copper, and the mode of suspension is *essentially* similar to that of Coulomb. The length of the skein from the magnet to the point of suspension is nine feet, and the detorsion plate is supported by braced frame-work, fixed firmly by brass bolts upon the two pillars which formerly carried the Kew transit instrument. The interior and exterior cases containing the magnet are coated entirely with gold paper, as are those of the Greenwich magnets.

A light conical brass tube projecting about six inches beyond the north end of the magnet, is affixed to the lower side of the stirrup which carries the magnet. Into this end slides (for adjustments) a small cylindrical tube, and from the extremity of the latter descends, vertically, a very small blackened wire (called the index) which passing through slits (long enough for its free motion always) cut through the bottoms of the cases, takes the place of the above-described electrometer, &c. in a lucernal microscope below.

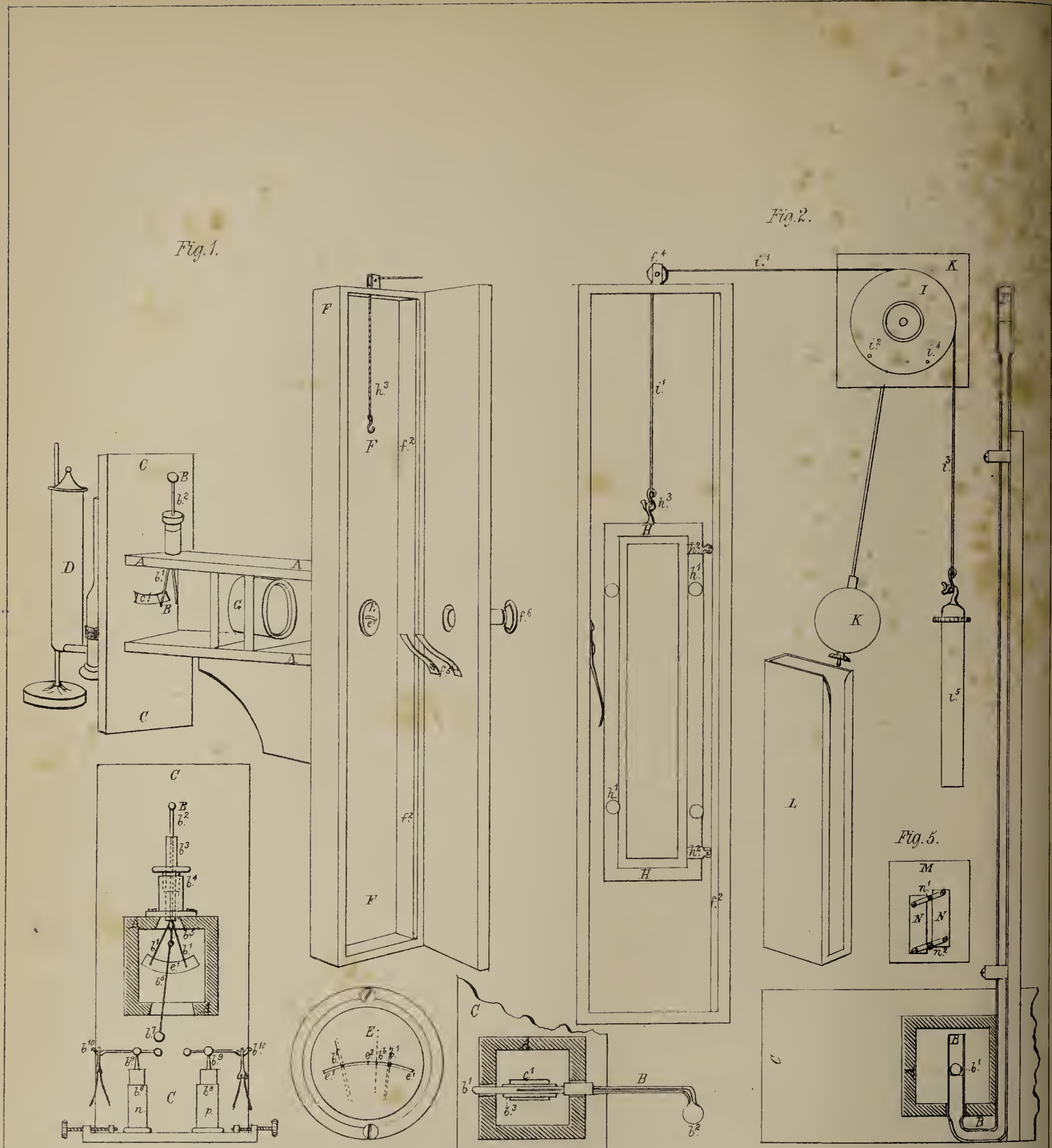
The lucernal microscope is in this instance much longer than before, in order that the motion of the index may be considerably magnified in its image, without sensible error of aberration, &c. The long case (now vertical) with its sliding frame, &c., have the usual form and dimensions; and the clock, with brass pendulum, weight, &c., is placed at a proper distance from the magnet.

I have taken great pains to prevent internal and external currents of air, but the magnet is far from being as steady as the Greenwich magnets, particularly during strong westerly and north-westerly winds. If it were as little subject to mechanical disturbances as the Greenwich magnets, I feel quite sure that the photographs would present as sharp outlines as those of the barometer, electrometer, &c.

Concerning the impressions which have been submitted to the Astronomer Royal's inspection, and compared with the readings of the Greenwich declinometer, I am permitted kindly by Mr. Airy to say, that the agreement of those results with such readings is highly satisfactory.

Postscript.—January 7, 1847.

Since writing the above I have felt strongly impressed with the great advantage which would result in these kind of registrations of the barometer, if that instrument could be rendered *accurately* self-corrective for temperature; and I have been much occupied with a project of this kind (promising very fairly). It consists in applying a solid metallic thermometer in such manner as to cause the whole barometer to descend exactly as much as increments of temperature cause the mercury to ascend.



J. Basire sculp.

Explanation of the Plates.

PLATE X.

Fig. 1. *Electrograph.*

A. The body of the lucernal microscope; its nearest side removed.

B. The voltaic electrometer; the pendulums b^1 suspended by hooks from an insulated piece b^2.

C. A board fitted to the end of A, and having a curved diaphragm (c^1) cut through it. The condensing lens is placed beyond this diaphragm (and not visible).

D. The Argand fountain lamp, or a camphine lamp.

E. The screen, with its slit (e^1).

F. The case which receives the sliding-frame. f^5. Springs pressing the glass of the frame (H. fig. 2) against E when the door is closed.

G. The tube containing the achromatic lenses.

Fig. 2.

H. The sliding frame suspended in F. h^1, h^1, &c. Bolts and nuts for pressing the photographic paper between the two plates of glass. h^2, h^2. Friction-rollers. f^3. Spring for pressing H against f^2, the ruler. h^3. Hooks.

I. The pulley on the hour-arbor of the clock. i^1. The line (of gut) suspending H; i^5. the counterpoise (to H) suspended by i^3; i^2, and i^4, terminations of i^1 and i^3.

K. The time-piece.

L. The transfer-box.

Fig. 3. *Thermometrograph.*

A. Section of the lucernal microscope.

B. Thermometer; b^1, its tube; b^2, its bulb. b^3. Diaphragm fixed upon b^1.

Fig. 4. *Barometrograph.*

A. Section of the microscope.

B. Siphon barometer. b^1. Black pith-ball resting on the mercury.

Fig. 5. *Contracting Diaphragm.*

M. Plate having a slit cut through its central part.

N. Small parallel ruler (of the common kind). n^1 and n^2. Screws which pass through the centres of its cross-bars (with friction) and screw firmly into M in the line of the centre of the slit.

Fig. 6. *Distinguishing Electrograph.*

A. Section of the microscope.

B. The voltaic electrometer. b^1. The pendulums suspended from the piece b^2. b^5. A little plate attached to the lower end of b^2 between the pendulums. b^6. A longer pendulum suspended by a hook from b^5, and

passing through a large aperture in the bottom of A. b^7. A pith-ball fixed on b^5. b^8, b^8. Two small Leyden jars, of *very* thin glass, fitted upon sliders, which can be made to approach or recede from each other by means of adjusting screws. b^9, b^9. Wires connected with their interior coatings, and capable of adjustment for height. b^{10}, b^{10}. Cross wires, each carrying at one end a brass ball, and at the other a small electroscope, of straws.

When this distinguishing electrometer is in use, the jars are charged artificially, one negatively, the other positively: consequently, if B receives a positive charge from the atmospheric conductor, b^7 is attracted toward the negative ball, and b^6 inclines in that direction, and *vice versâ*.

It is evident, therefore, that in the resulting photograph a line must appear between the two lines, produced by the images of the short straws b^1, b^1, but nearer to one than to the other. In order to ascertain with certainty, in low intensities, to which ball it inclines, a very small bar (e^2, fig. 7) is placed across the exact centre of the slit in the screen; the *shadow* of which bar (e^2) of course creates a central line in the photograph.

These jars retain a *sufficient low* charge for twenty-four hours even in a humid state of the air (for they are somewhat similarly circumstanced to an electrophorus, which will frequently retain a low charge for one or *two* months).

Fig. 7. The screen E (*vide* fig. 1) with its slit e^1, the visible parts of the images of the short straws b^1, b^1, that of the long straw b^6, and the bisecting little cross-bar e^2.

PLATE XI.

Fig. 8. *The Declination Magnetograph.*

AV. A box divided into two compartments.

W. An interior box.

A. The lucernal microscope.

B. The declination magnet. b^2. Its stirrup, from which it may be removed at pleasure, and the usual brass bar substituted after relaxing two milled-headed screws and turning downward the nearest sides of the stirrup. b^3. The pair of light tubes connected with the stirrup by means of a short tube, which permits a horizontal adjustment of b^3. The counterbalance on the nearer end of b^3 is also adjustible.

b^1. The index. b^4. The damper, the central parts of which, above and below, are expanded and form rings.

Fig. 8.

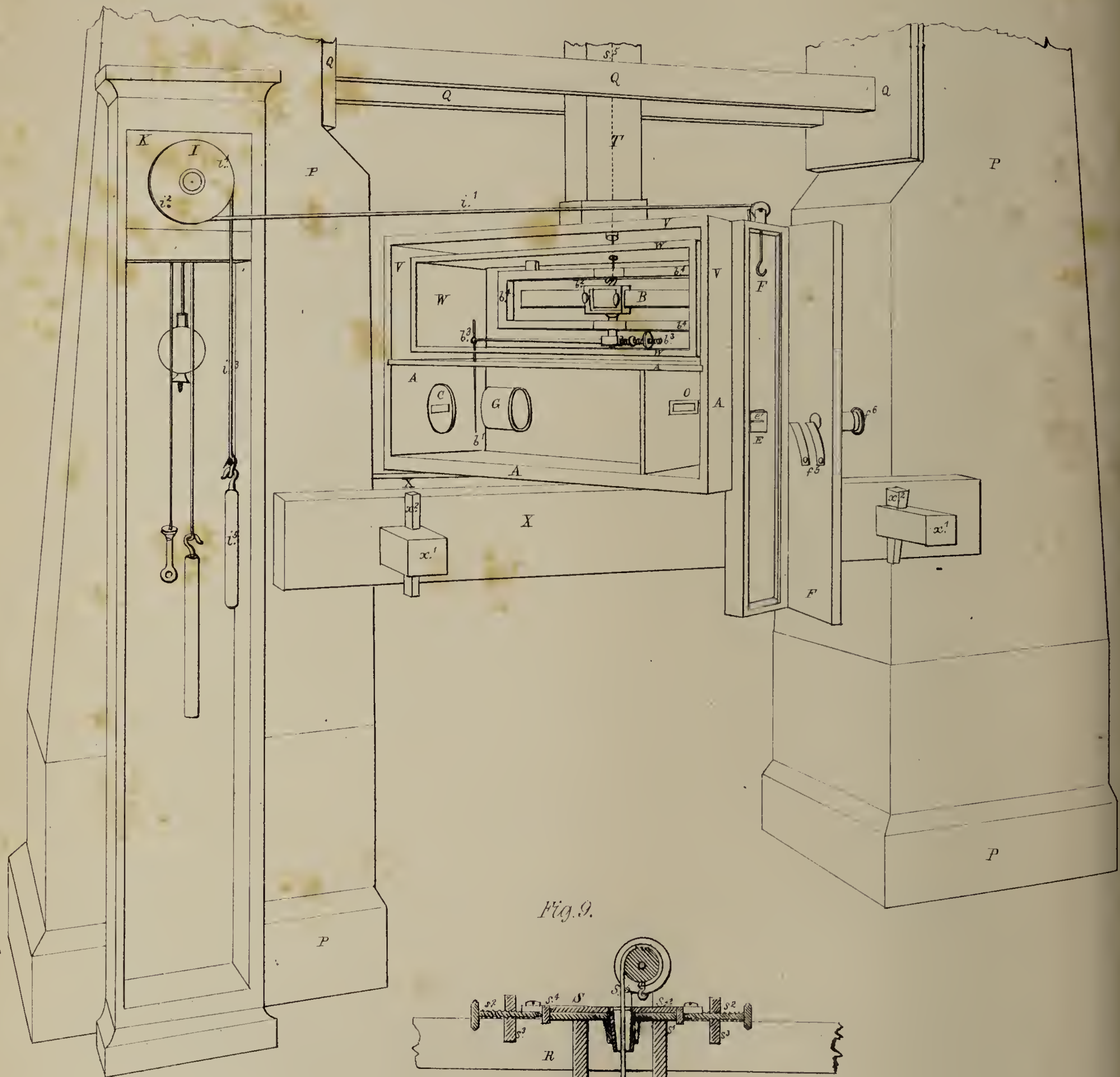


Fig. 9.

F.R. del.

J. Basire sculp.

C. The condensing lens (or a glass plane), beyond which a brass lamp (*vide* fig. 1), or when daylight is used, a mirror is placed.

G. The tube containing the achromatic lenses.

O. A diaphragm.

E. The screen. e^1. The slit cut through it.

F. The case for reception of the sliding frame (*vide* H, fig. 2).

IK. The pulley, clock, &c. (*vide* fig. 2.)

PP. The two pillars of the late transit instrument at Kew.

QQQ. Parts of the frame work and braces fixed upon PP.

XX. Bearers clamped firmly upon PP by means of cross-bars, x^1, x^1, and wedges, x^2, &c.

A plumb-line, not visible in the sketch, is suspended from the summit of the frame-work and descends to X for the purpose of ascertaining whether any appreciable movement of the point of suspension has occurred. I have not yet discovered any such movement.

Fig. 9.

R. One of the two bearers which unite the upper opposite ends of QQ (fig. 8).

S. Section of the detorsion-plate, &c. s^1. A rectangular plate which can be slid between grooved pieces fixed upon the bearers (R) and accurately adjusted by means of the screws s^2, s^2 passing through the pieces s^3, s^3. s^4. The graduated circular plate with its central neck descending through a socket in s^1*. On s^4 a pulley is supported, which is provided with milled-headed nuts on its prolonged axis, serving to secure it from revolution at pleasure. s^5. The silken skein attached to the pulley and descending through a small aperture in the neck of s^4.

T. The upper part of a narrow box inclosing the skein. The lower part is seen at T, fig. 8.

* I propose to avail myself of Sir John Herschel's obliging suggestion of turning, with *extreme* slowness and equability, this detorsion-plate on its centre by means of a tangent screw, moved by simple apparatus worked *below*, and whilst the effect on the brass bar may be watched by the operator, even when the boxes are closed.

PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXLVII.

PART II.

LONDON:

PRINTED BY RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET.

MDCCCXLVII.

Adjudication of the Medals of the Royal Society for the year 1847 by the President and Council.

The Copley Medal to Sir John Frederick William Herschel, Bart., F.R.S., for his work entitled "Results of Astronomical Observations made during the years 1834, 1835, 1836, 1837 and 1838, at the Cape of Good Hope; being a completion of a Telescopic Survey of the whole surface of the visible heavens, commenced in 1825."

The Royal Medal in the department of Chemistry, to George Fownes, Esq., F.R.S., for his Papers entitled "An Account of the Artificial Formation of a Vegeto-alkali," and "On Benzoline, a new Organic Salt-base from Bitter Almond Oil," published in the Philosophical Transactions for 1845.

No recommendation of the Royal Medal in the department of Mathematics having been received, it was awarded to William Robert Grove, Esq., F.R.S., for his Papers entitled "On the Gas Voltaic Battery.—Voltaic Action of Phosphorus, Sulphur and Hydro-carbons," and "On certain Phenomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat," published in the Philosophical Transactions for 1845 and 1847.

The Bakerian Lecture for 1847 was delivered by William Robert Grove, Esq., M.A., F.R.S., and is contained in his paper entitled "On certain Phenomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat."

CONTENTS.

Appendix.

PHILOSOPHICAL TRANSACTIONS.

XI. *On the Amount of the Radiation of Heat, at night, from the Earth, and from various bodies placed on or near the surface of the Earth.* *By* JAMES GLAISHER, *Esq., of the Royal Observatory at Greenwich. Communicated by* G. B. AIRY, *Esq., F.R.S., Astronomer Royal, &c.*

Received January 14,—Read February 4, 1847.

IN the Report of the Committee of Physics of this Society in the year 1840, the measure of the radiation of heat at night from the earth was specially mentioned as being of importance to meteorology; and it may be added that it is of the utmost importance to the economy of nature.

As soon as the duties of the Magnetical and Meteorological Observatory permitted, I employed some time in making myself acquainted with all that was known upon the subject of radiation. The results of my researches were only a few papers in the Transactions of this Society, and the Treatise on Dew by Dr. WELLS; and in general I found a great want of good observations; I therefore determined to pursue the subject with some degree of steadiness. The instrument recommended to be used in the report above referred to, was a self-registering minimum thermometer, placed in the focus of a polished metallic reflector; and, in consequence of this recommendation, the instrument has been in very general use; it was, however, mentioned as being an imperfect instrument.

The instrument being thus confessedly imperfect, it became exceedingly desirable to ascertain the amount of its errors; and, to this end, to have very many comparisons made between it and those instruments or methods which might be found less imperfect, so as to have a ready and certain means of converting the results derived from observations by it to other results, if it were found desirable so to do. With this view the thermometer, whose bulb was placed in the focus of a reflector, has been read, almost without exception, in every series of experiments.

My first object was the obtaining instruments of the best possible kind, and I con-

sidered that the essentials of the thermometers adapted to the investigation were threefold, viz.—

1st. That the points corresponding to the freezing and boiling of water be exactly determined.

2nd. That the column of mercury which fills the tube be exactly uniform throughout.

3rd. As bodies exposed to the sky must radiate as much heat to it during the prevalence of wind as they would do if the air were still, and as I had always found, during the continuance of the strongest and most steady winds, that there were periods of calms of some duration, it was necessary that the thermometers should be of the most delicate kind, and the most sensitive to the variations of heat, so that they would mark a superficial or transitory cold, as, if not, the frequent application of warm air in windy weather would quickly return a heat equal to that lost by radiation.

With the view of obtaining instruments combining these essentials, I placed myself in correspondence with Mr. Watkins, optician, in the year 1842, described to him the kind of instrument I wanted, and the purpose for which I required them, and after this time I frequently received different thermometers from him for the purpose of experiment. I was thus occupied during many months in making these experiments upon thermometers whose stems were varied in their lengths, some embracing an extent of upwards of 212°; others of a less extent, but whose lengths for a degree were different; to some of these divided scales were applied, varied in their kind and form, hinges being furnished to several, so that a part of the stem and the bulb were free; and others were without any scales affixed to them whatever, the divisions being cut on their own stems; other experiments were made upon thermometers whose bulbs were varied with respect to their form, size and colour; and in general, the results of the experiments tended to the obtaining of instruments which would give the most accurate results.

The following are some of the results of these experiments:—

Being desirous of testing the points 32° and 212° myself, so that I could determine their errors, as also the errors of the other parts of the scale by means of Bessel's formula (Konigsberg Astronomical Observations for 1821, p. 9)*, I was anxious for instruments whose extent of division should embrace these points; I soon, however, abandoned this idea, as it would have required thermometers with very long stems, a circumstance I found materially to affect their readings, which varied with every different inclination of the thermometer to the horizon, and were correct only when placed horizontally. I did this the more readily as I was in possession of an instrument which had been made for me by Messrs. Watkins and Hill ten years previously, the point corresponding to 32° of which had been examined every year, and found to be correct; the readings of this instrument had also been compared with the best standard instruments we possess, and found to agree with them at every part

* See a full illustration of this method in Kupffer, Annuaire Magnétique et Météorologique for the year 1841, pp. 41 to 51.

of the scale; I therefore determined to use this as a standard with which to compare every thermometer.

I also found that it was absolutely necessary to have the division marked on the thermometer stems themselves; for those to which scales were affixed, even those furnished with hinges, so that a part of the stem and the bulb were free, and those whose bulbs projected beyond the scale could not be laid so nicely on different substances as those without scales; for that and other reasons all the thermometers used in the following experiments were without scales affixed to them, the divisions being engraved and coloured upon the stems themselves.

In the course of the experiments I found that circular bulbs were the more sensible in proportion to their smallness, but with a bulb sufficiently small to have the desired sensibility, the column of mercury was so fine that it would have been impossible to observe accurately with it at night in the position in which the instruments were necessarily placed in these observations.

The length of the thermometer finally used was thirteen inches, including the bulb, whose diameter was a quarter of an inch, and length three quarters of an inch (as shown in the figure); therefore, as the instruments were divided from 0° to 130°, each degree was about 0·1 inch in length; these were used during the night observations, and they were left on their respective substances till about 9 o'clock in the morning, until it was found that many of them were broken; the absorptive power of grass and the filamentous substances being such that before this time a temperature of more than 130° had taken place even in the month of April; other instruments were afterwards constructed both of the same length and of the same form, and graduated as far as 160° or 170°. These were occasionally used during the night observations, and always during those of the day in experiments upon the absorptive powers of different substances.

These instruments were so delicate that on taking them from air of the temperature of 60° to that of 37°, the latter temperature was indicated in about two minutes; and therefore if at any time a lull took place in a gale of wind of two minutes' duration, or even less, the amount of heat lost by radiation under the then state of the sky would be correctly registered by these instruments.

On September 13, 1843, I received twenty-five mercurial thermometers of the above form, and as many self-registering minimum thermometers, with circular bulbs, whose divisions were also on their own glass stems; and at this time I commenced the regular series of observations with the mercurial thermometers; that with the minimum thermometers had been begun long before. A year was consumed in these preliminary experiments, and in ascertaining the precautions necessary to obtain correct determinations; in consequence of several of these being neglected before this time, the previous observations must be regarded as undeserving of confidence; the results from them therefore have been omitted in the following Tables.

I now proceed to speak of the comparison of the thermometers with the

standard. Among others received in January 1843, were five of the form finally adopted for use, and the following are the comparisons with the standard, made by hanging them in the air and near to the latter :—

1843. Month, day, and hour.	Reading of standard thermometer.	Reading of thermometers.				
		a.	*b.*	*c.*	*d.*	*e.*
d. h.	°	°	°	°	°	°
Feb. 5 22	34·8	34·8	34·8	34·8	34·8	34·7
6 0	35·7	35·4	35·6	35·6	35·6	35·7
6 1	36·4	36·2	36·3	36·2	36·3	36·3
13 22	35·9	35·8	36·0	35·8	35·9	35·9
13 22½	36·4	35·9	36·0	36·1	36·2	36·3
13 23	37·0	36·8	37·0	37·0	37·0	37·4
14 1	39·0	38·8	39·0	39·2	39·2	39·2

The following comparisons were made of those received on September 13th by means of water of different temperatures in the following manner. Water of a higher temperature than needed was placed in a vessel, and the required temperature was obtained by the application of cold water, the two being well mixed together till the standard thermometer read the same at every part of the mixture. The thermometers were then all placed in it and parallel to each other ; they were thus allowed to remain for one hour, and their readings were taken quickly. Water of a different temperature was then prepared in a similar way, and so on successively. The comparisons occupied an entire night, and during this interval of time the temperature of the room was kept uniform, in fact it did not vary one degree.

This method was found to be very troublesome, and to ensure accuracy a long time was required to be devoted to it. The mixing of water of different temperatures together, so that the whole mixture became of the same temperature, from the circumstance of its being so bad a conductor of heat, was found to be difficult. After this time I adopted the following method in all subsequent comparisons :—

1st. The water was heated to a temperature above the highest required.

2nd. The standard and all the thermometers for comparison were placed, in this water, parallel to each other, and after some time their readings were taken as quickly as possible.

3rd. The readings were afterwards taken as the temperature of the water declined every 2° or 3°, till the temperature of the water was at or near that of the apartment; in case the decline of temperature was found to be too rapid, the surface of the water was covered by flannel.

4th. A quantity of water was cooled to a point lower than the lowest required temperature, by means of ice, or of ice and salt, and the thermometers were placed in this as before.

5th. The readings of all the thermometers were taken as this mixture increased 2° or 3° in temperature, till it was at or near the temperature of the apartment, its surface being covered by flannel if the increase of heat was thought to be too rapid.

By this means the comparisons were made with ease and certainty, and they readily indicated every inequality of the thermometer tubes.

1843.	Greenwich mean time Oct. 21^d.				
Name or letter of reference of the thermometer.	7^h 30^m.	11^h 30^m.	15^h 30^m.	17^h 40^m	Correction to be added to readings of the thermometers.
Standard.	45°·1	49°·8	69°·3	90°·5	
A.	45·1	49·8	69·3	90·5	
B.	45·1	49·8	69·3	90·5	
C.	45·1	49·8	69·2	90·5	
D.	45·0	49·8	69·3	90·4	
E.	45·1	49·8	69·3	90·3	
F.	45·1	49·8	69·3	90·4	
G.	45·0	49·8	69·3	90·4	
H.	45·1	49·9	69·3	90·5	
I.	45·1	49·8	69·2	90·4	
J.	45·0	49·7	69·3	90·4	
K.	45·0	49·7	69·3	90·4	
L.	45·0	49·8	69·3	90·4	
M.	45·0	49·8	69·3	90·4	
N.	45·1	49·8	69·3	90·5	
O.	45·1	49·9	69·4	90·5	
P.	44·6	49·4	69·0	90·0	Add 0°·5 to all readings.
Q.	44·9	49·6	69·2	90·3	Add 0°·2 to all readings.
R.	45·0	49·8	69·3	90·5	
S.	44·8	49·5	69·0	90·2	Add 0°·3 to all readings.
T.	44·8	49·5	69·0	90·2	Add 0°·3 to all readings.
U.	45·1	49·8	69·2	90·5	
V.	45·1	49·6	69·2	90·5	
W.	45·1	49·7	69·1	90·5	
X.	45·1	49·8	69·1	90·5	
Y.	45·1	49·6	69·0	90·4	

During the operation of comparing the thermometers, it was found absolutely necessary to have all of them in the same position with respect to the vertical, for it sometimes happened that their readings varied with their position; this variation with the self-registering minimum thermometers was so great that I could not compare them at this time; afterwards I procured a shallow vessel of sufficient extent to receive them horizontally; in this vessel the comparisons were taken, and their readings were found to agree with those of the standard to 0°·1 or 0°·2, the same thermometer being frequently as much in excess at one part of the stem, and as much in defect at another.

The possession of upwards of fifty instruments whose extreme difference of reading from the standard was a constant quantity of half a degree in one thermometer, and a constant quantity of 0°·2 or of 0°·3 in three others, the remainder being absolutely free from error, was exceedingly gratifying. I felt that all the time and trouble I had bestowed on them was well repaid. I had not expected to be so successful in obtaining so many essentially good thermometers, and I had prepared myself to ascertain their errors at every division by Bessel's formula, which, although implying a long process, gives good results; all this trouble, however, I was saved, and also

all the work which would have been necessary in applying the correction, had such been needed.

The construction of such instruments must be considered as highly creditable to Messrs. Watkins and Hill, and I feel that I should be doing an injustice to Mr. Watkins, did I not here mention my obligations to him for his readiness at all times to meet my wishes, which I fear were sometimes troublesome.

The observations were made in the grounds attached to the Royal Observatory, being S.S.E. of the Magnetic Observatory, and distant from it about forty feet.

The form of the piece of ground is nearly square; its extent about 10,000 square feet, and its surface nearly level. At one end is the extreme south arm of the Magnetic Observatory, whose height is twelve feet and breadth thirteen feet; at fourteen feet north of this arm, the two east and west arms project each fourteen feet, and their height is the same as the south arm. On each of the three remaining sides, at the distance of about forty-five feet, is a close wooden fence, between five feet and six feet high. At the distance of ninety feet north-west is a fine and spreading oak-tree, and a little beyond it are other trees but of a less elevation. On the east and near the outside of the fence are chestnut-trees. All these circumstances had influence on the readings, and caused them to be higher (particularly those that were made on or near the surface of the ground), and therefore the differences of the readings, as compared with those in the air, were less than they would have been if the surrounding fence and trees had been further removed, or the observations had been made in a wide and open plain, and I have reason to believe to a much greater extent than would at first appear to be the case.

A portion of the grass plat containing 210 square feet, whose nearest part was twenty-eight feet south of the Magnetic Observatory, was enclosed by low and open palings; within this enclosure the self-registering minimum thermometers were placed after April 1843, and some of them continued to be regularly observed till the present time (1847). At one angle of this enclosure a piece of board six feet long, four feet wide, and $1\frac{1}{4}$ inch thick, was elevated three feet above the grass plat, by means of four props of equal height; upon this raised board all experiments upon substances in the shape of powder were made.

I now proceed to speak of some of the substances upon which experiments have been made. Those on the temperature of grass were always made both on long and on short grass, the blades of the former being bent by strong pressure towards the earth, and overlapping each other so as to completely cover the surface of the ground; the blades of the latter, being less than an inch in height, were erect and stiff; in this respect only did the latter differ from the former, each portion being a part of the same grass plat, separated from each other by a few feet only, and therefore exposed to the same portions of the sky. The metallic plates used had generally a surface of more than 100 square inches. The thermometers for ascertaining the temperature of the air at distances from one inch to twelve feet from the earth, were placed with

their stems passing through a piece of wood planted firmly in the ground, and whose thickness was two inches and breadth three inches, with the bulbs at least nine inches from the wood.

Bodies in the state of powder were generally in quantity such that about thirty square inches of surface were exposed, and their thickness was about half an inch. The filamentous substances exposed a surface of about 100 square inches, and their thickness was about half an inch. With respect to the other substances, the circumstances under which the observations have been made are sufficiently explained in the section of observations. The thermometer was laid on long grass in such a manner that the bulb was not covered by a single blade; on short grass it rested on the fork of two Y's, so that the bulb was sustained on the top of the grass; on metallic plates it was so laid that the bulb occupied the centre of the plate; on substances in powder it was so placed that the bulb occupied the centre of the mass, and just in contact with it, the stem being supported by pieces of wood; on filamentous substances the bulb occupied the centre, and care was taken that it did not sink within, or was covered by any portion of the substance; and the same plan was adopted throughout all the experiments.

During a series of observations the thermometers were frequently removed from one substance to another; those whose readings were the highest were interchanged with those which were the lowest, and so on: in the case of observations of the same kind of metals at different heights, or of different thicknesses, or indeed in any specific comparison, they were always interchanged among themselves; it is possible that had the same instrument remained on the same substance, or in the same position, a more even set of readings in some cases might have been made: but this interchange of instruments removed all doubt as to the cause of the differences in the readings being altogether due to the difference of position, or to the inherent quality of the body itself upon which the instrument was placed. Some of the other precautions used in taking the observations were as follows. First, the observer approached the instrument at that end which was the most distant from the bulb, and held his breath during the time; and the readings were taken as quickly as possible, so that no heat should be imparted to them from the observer's body. In consequence of their divisions being cut on glass, a difficulty was at first experienced in reading them without placing the reading-lamp too near to the instruments: habit after some time enabled the observer to place the lamp so that the divisions were instantly perceived. Another and a greater difficulty was that of readily seeing the top of the mercury, in consequence of the fineness of its column: after various experiments it was found that this became readily seen by slipping a piece of coloured card under the instrument at about the place where it was known the column terminated: the colours found to answer best were drab and yellow. I may mention here that attention was paid to every precaution that experience suggested, or reflection pointed out as desirable.

Each set of observations may be looked upon as a series of experiments made for the purpose of ascertaining the different tendencies of various bodies to become cold upon exposure to a cloudless sky at night. To many persons it must be a new fact, that a perfectly dry body, placed in contact on all sides with other bodies of the same temperature with itself when the sky is covered by clouds, shall on the sky becoming less cloudy or cloudless, become much colder than those bodies, to so great an amount as is exhibited in the following tables, and that it shall remain so for many hours; yet these circumstances were exhibited in every series of observations*.

The formation of dew was found to depend solely on the temperature of the bodies upon which it was deposited, and it never appeared upon them till their temperatures had descended below that of the dew-point in their locality, as found by observations of a dry and wet bulb thermometer placed in their vicinity.

The amount of water thus deposited was the greatest upon the substances whose temperatures were the lowest: among these bodies glass was found to radiate heat freely, and it very readily became wet with dew. In consequence of this property, the tube of a naked thermometer, which was lying on a substance entirely free from moisture, was frequently found covered by dew, and therefore it seemed probable that the temperature exhibited by the instrument was not that of the body in question. On such instances occurring an attempt was made to correct the error by enclosing the thermometer stem in a tube made of gilt paper; the bulb alone, resting on the substance, remained exposed to the sky. The differences between the readings of a thermometer thus enclosed, and when naked, were found to be sensible, but small in amount; it was observed that when the thermometer was wholly naked, the stem was at times wet when the bulb itself was dry; and at all times much less moisture appeared on the bulb than on the stem, unless the disposition of the substance in question to become cold was the same, or greater than that of glass. The error arising from this cause was chiefly confined to the consequent contraction of the mercury in the stem, and not in the bulb, and which was considered to be avoided by the use of gilt paper: the error in all cases must have been small. It was found that the differences between the temperature of the air and of bodies on the earth, at night, in equally calm and clear weather, was the same at every period of the year, but it was found that the amount of dew deposited during such times was much greater in summer than in winter. This is easily accounted for, from the now well-known relation existing between temperature and moisture. At all seasons of the year, at night, the depression of the temperature of the dew-point below that of the air is small, or the air is in a state of saturation nearly, and therefore in summer a certain diminution of temperature would cause much more vapour to be changed into water, than an equal diminution in winter would do.

Radiation of heat from the earth to the heavens must exist at all times both day and night, and in all states of the sky. Generally, when the sun is above the

* The whole of these observations are placed in the Archives of the Royal Society.

horizon, the heat emitted by it to the earth will overbalance that which the earth radiates upwards; at times however, in places shaded from its direct beams, the amount of heat radiated exceeds that received from the sun and all other sources, and dew will be continually deposited throughout the day. Some instances of this are exhibited in the following Tables.

In taking these observations I have been much assisted by my then colleagues in the Magnetical and Meteorological Department of the Royal Observatory, viz. Messrs. Dunkin, Hind and Paul: these gentlemen frequently, on my commencing a series of observations, continued them as long as circumstances required, or they have begun a series which were continued by myself; and whenever any doubt attached to the readings from the unexpected lowness of the thermometers or other causes, they were always confirmed by one or other of these gentlemen, and afterwards by an interchange of instruments.

I have also to acknowledge my obligations to the Astronomer Royal for his kindness in permitting me to carry on these experiments within the grounds of the Royal Observatory; also for providing me with a skeleton form in which the observations were registered; and also for inclosing the piece of ground within which the registering instruments were placed.

The whole of the calculations have been twice performed by myself at different times, and parts of them, which appeared to be more liable to error than others, have been examined by another person. I believe, therefore, the whole to be nearly correct.

Section I.—*Results of Simultaneous Observations made by Mercurial Thermometers, not self-registering.*

The first process in the reduction of these observations, was to take the difference between the reading of the thermometer, freely suspended in air, at the height of four feet, and protected from the effects of radiation, and the simultaneous reading of every other thermometer.

The next process was to divide these differences into groups, arranged according to the excess of the reading of the thermometer, suspended in the air, above that placed on long grass.

The next step was to collect all these differences under the head of their respective substances for every degree of such excess of air temperature above that of long grass temperature.

The next step was to arrange these numbers according to the dates of their occurrence, and to write out abbreviative remarks which were made at the time of observation; and in this way the following Tables have been formed.

Tables I. to XVI. contain the results deduced from the observations taken between 1843, September 13, and 1843, November 15; Tables XVII. to XXVIII. contain the results from the observations taken between 1843, November 16, and December 31; and Tables XXIX. to XLIV. contain those from observations taken between 1844, January 1, and 1844, May, 1.

Excess of the reading of a thermometer placed in air at the height of 4 feet, protected from the effect of radiation, above the readings of thermometers placed on different substances fully exposed to th

Number of the Tables.	1843. Astronomical day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air above that placed On long grass.	On short grass.	On raw wool on grass.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On garden mould.	On gravel.	On river sand.	On river sand on the raised board.	One inch high above grass.	Two inches high above grass.	Three inches high above grass.	Six inches high above grass.	Twelve inches high above grass.	On unwrought white cotton wool on grass.	On unwrought white cotton wool on raised board.	On raw wool on raised board.	On flax on raised board.
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
I.	Sept. 13. 19 20	54·8	−2·2	−2·2	...	...	...	...	...	...	...	...	0·0	−2·2	−2·2	−2·2	0·8	......	...	...	−2·2	−2·2	...	...
	17. 19 20	61·5	−0·7	−0·2	...	...	−1·0	...	...	...	...	−1·0	0·0	−0·5	−1·3	−1·2	...	1·7	...	...	...	...	...	...
	22. 18 0	52 2	−1·1	−0·8	...	...	0·0	...	...	...	...	...	...	−1·4	...	0·4	...	0·4	...	...	−2·3	...	...	...
	22. 19 20	54·2	−2 3	−1·1	...	...	−0·8	...	...	...	...	...	...	−1·8	...	0·2	...	0·9	...	...	−3·8	...	...	...
II.	24. 18 40	49·0	0·2	0·9	...	...	2·0	1·0	...	...	...	...	...	0·0	...	1·0	...	1·7	...	...	−0·8	...	...	...
	26. 19 20	44·5	0·8	1·3	...	...	0·5	...	...	...	...	...	...	−1·7	...	0·5	...	1·3	...	...	−2·7	...	...	...
	Oct. 15. 19 20	32·0	0·4	1·7	2·4	2·2	−0·1	1·3	...	...	...	...	...	...	...	1·7	...	...	1·2	...	2·2	...	...	...
III.	Sept. 24. 18 15	48·7	1·3	1·6	...	...	2·6	1·6	...	...	...	−0·1	...	...	...	1·7	...	1·8	...	...	0·4	...	...	...
	26. 17 35	40·8	1·7	1·4	...	...	2·4	...	...	...	...	...	...	0·3	...	1·2	...	1·6	...	...	1·0	...	...	...
	Oct. 16. 14 50	40·0	1·0	1·0	1·0	1·0	2·0	0·5	...	...	...	...	...	...	...	1·0	...	1·0	...	...	1·0	...	...	...
IV.	Sept. 18. 17 30	60·3	2·5	2·1	...	...	...	...	...	...	...	1·3	2·5	2·3	...	2·3	...	1·7	...	...	2·0	...	...	...
	Oct. 3. 11 35	58·3	2·6	2·3	...	...	1·7	0·4	...	...	...	1·8	...	...	...	1·6	...	...	0·7	...	2·5	...	...	...
	4. 11 20	55·7	2·7	2·7	...	...	1·9	1·7	...	...	...	...	...	2·7	...	2·7	...	...	2·7	...	2·5	...	...	...
	12. 18 25	38·5	2·5	2·5	2·0	2·5	2·5	2·5	...	...	...	...	...	...	...	2·5	...	...	2·0	...	2·5	...	...	...
V.	Sept. 20. 18 20	50·7	3·7	3·0	...	...	2·5	...	...	...	...	1·5	...	...	...	1·9	...	2·1	...	...	4·7	...	...	...
	21. 13 20	57·2	3·2	3·0	2·9	...	0·7	...	...	...	...	...	...	2·6	...	2·1	...	0·6	...	...	...	...	...	...
	22. 15 30	50·5	3·3	2·9	1·3	...	2·9	1·1	...	...	...	...	...	2·3	...	3·3	...	1·2	...	...	...	...	...	...
	Oct. 12. 17 15	38·0	3·2	3·2	5·0	5·0	3·0	2·0	...	...	...	...	...	...	...	3·0	...	...	2·0	...	5·0	...	...	...
	13. 15 20	41·5	3·5	3·0	3·0	2·2	2·7	0·9	...	...	...	...	...	...	...	1·9	...	...	1·2	...	3·0	...	...	...
	18. 21 0	38·2	3·0	3·0	5·5	4·0	4·5	...	...	...	...	2·7	2·8	0·8	...	3·0	...	...	1·0	0·8	...	3·5	...	...
	27. 5 0	44·0	3·3	1·8	2·2	2·9	2·0	1·0	−3·0	−1·5	...	0·8	−0·7	0·4	...	2·0	...	...	1·0	0·9	2·7	...	...	...
	Nov. 3. 7 40	48·1	3·1	3·8	6·7	6·1	3·9	1·1	−0·5	−2·1	...	1·9	2·1	3·3	...	3·1	...	...	0·5	−0·1	5·0	...	...	...
	4. 7 0	42·5	3·8	2·0	7·0	6·3	2·7	1·5	−5·5	−1·0	...	−0·5	−2·5	−0·6	...	1·5	...	...	0·7	0·2	3·0	...	...	...
	9. 6 30	32·5	3·5	2·7	2·0	2 0	2·5	0·9	−6·0	−2·3	...	0·5	0·7	0·5	...	...	...	...	−0·3	−0·2	3·5	...	...	...
VI.	Sept. 13. 15 37	49·5	4·5	4·5	...	...	6·5	...	...	...	...	...	0·5	4·5	4·0	4·5	3·0	...	...	...	4·5	4·5	...	...
	21. 9 10	60·7	4·7	4·7	...	...	3·9	...	...	...	...	...	...	4·3	...	4·3	...	2·4	...	...	4·5	...	...	...
	Oct. 3. 15 20	55·6	4·9	4·0	...	...	3·6	2·4	...	...	...	3·2	...	...	...	3·3	...	...	2·4	...	7·3	...	...	...
	5. 11 0	54·5	4·5	4·1	...	...	3·5	2·0	...	...	...	3·3	...	...	...	3·3	...	...	2·5	...	...	...	...	...
	13. 11 0	41·5	4·8	4·5	4·7	3·5	4·3	2·0	...	...	...	...	...	...	...	3·5	...	...	1·5	...	4·5	...	...	...
	13. 20 40	40·5	4·5	3·5	8·5	4·5	2·5	...	...	...	...	...	...	...	...	2·5	...	...	1·5	...	8·5	...	...	...
	20. 11 20	42·3	4·8	3·8	5·8	5·5	3·8	0·8	−0·7	0·3	...	1·5	1·8	1·5	...	2·3	...	...	0·1	0·1	5·1	...	...	...
	20. 13 0	44·8	4·0	1·8	3·6	4·0	1·8	0·0	−1·3	1·3	...	0·8	1·0	0·8	...	1·6	...	...	−0·2	−0·2	2·6	...	...	...
	23. 7 30	49·2	4·3	3·0	5·2	5·0	3·6	0·7	0·0	0·5	...	1·2	1·1	2·0	...	2·4	...	...	0·0	−0·6	4·2	...	...	...
	Nov. 3. 10 30	52·1	4·1	2·3	4·6	6·1	3·6	0·6	−3·3	4·0	...	2·9	3·6	3·3	...	3·9	...	...	0·6	0 1	4·6	...	...	...
	7. 9 0	43·1	4·1	5·9	9·1	7·9	5·3	2·3	−3·0	1·9	...	2·3	2·1	1·6	...	3·5	...	...	0·3	0·1	6·1	...	...	...
	15. 5 10	37 0	4·0	4·0	4·4	4·0	4·2	1·8	−3·0	...	...	...	...	...	...	...	...	...	1·0	0·2	4·0	...	...	...
VII.	Sept. 13. 15 24	50·0	5·5	6·0	...	...	8 0	...	...	...	...	...	1·0	6·0	5·0	6·0	3·5	...	...	...	6·0	5·5	...	...
	13. 17 20	51·5	5·5	5·5	...	...	...	...	...	...	...	...	1·0	5·5	4·5	5·5	3·5	...	...	...	5·5	5·5	...	...
	17. 17 20	58·0	5·2	3·0	...	...	...	...	...	...	...	2·8	1·2	2·9	2·6	2·3	...	1·8	...	...	...	...	...	...
	20. 16 20	51·0	5·0	3·8	...	...	3·0	...	...	...	...	...	3·8	...	...	4·0	...	2·0	...	...	6·7	...	...	...
	20. 17 20	50·0	5·8	3·8	...	...	2·2	...	...	...	...	...	3·0	...	...	3·0	...	2·6	...	...	6·9	...	...	...
	Oct. 2. 9 20	57·2	5·9	4·7	...	...	3·6	1·2	...	...	...	...	...	3·6	...	3·4	...	...	1·8	...	5·2	...	...	...
	3. 11 10	58·3	5·8	5·7	...	...	3·5	2·3	...	...	...	2·7	...	...	...	2·8	...	...	1·9	...	6·3	...	...	...
	5. 9 30	53·5	5·2	4·5	...	...	3·5	2·0	...	...	...	......	...	3·2	...	3·5	...	...	2·5	...	...	...	...	...
	9. 18 0	39·2	5·2	3·8	...	...	2·7	2·4	...	...	...	−0·8	...	2·7	...	2·2	...	...	2·2	...	...	...	...	...
	12. 15 15	35·7	5·7	5·9	10·0	7·0	5·9	3·2	...	...	...	...	...	...	...	3·9	...	...	3·2	...	8·2	...	...	...
	16. 13 0	36·5	5·5	3·5	4·5	4·5	5·5	1·0	...	...	...	...	...	...	...	3·5	...	...	2·5	...	4·5	...	...	...
	23. 5 15	50·5	5·3	4 7	14·4	9·0	5·5	2·0	−0·4	1·0	...	2·3	2·0	3·5	...	3·3	...	...	1·0	0·3	12·7	...	...	...

Oct. 16, 14ʰ 50ᵐ; rain falling. Sept. 18, 17ʰ 30ᵐ; after a heavy rain. Sept. 20, 18ʰ 20ᵐ; after the sun had risen, clouds thin in zenith. Oct. 12, 17ʰ 15ᵐ rain. Oct. 18, 21ʰ 0ᵐ; sun high. Oct. 3, 15ʰ 20ᵐ; the stars are shining dimly. Oct. 5, 11ʰ 0ᵐ; a few stars have been visible occasionally east of zenith. O 13, 20ʰ 40ᵐ; the sun had risen more than two hours. Nov 3, 10ʰ 30ᵐ; nocturnal rising temperature of the air, and the reading of the barometer is decreasin Sept. 17, 17ʰ 20ᵐ; deposition of moisture. Oct. 9, 18ʰ 0ᵐ; clouds in every direction. Oct. 12, 15ʰ 15ᵐ; some rain had fallen.

Oct. 18, 21ʰ 0ᵐ. The observations at this time have not been used in subsequent calculations, they were taken after a long series of observations, extendi over the whole night. On examining the state of the different substances at this time, it was found that every fibre of cotton wool was encrusted with a beauti fringe of hoar-frost, and there were a few spikes of ice: raw wool was covered with lumps of ice, and there were some clear transparent flakes of ice, resembli

y, arranged according to the difference of the readings of the one on Long Grass and that in Air, from
servations taken between 1843 Sept. 13 and Nov. 15.

Excess of the reading of the thermometer in air above that placed																									Clouds.			Wind.		
on grass.	On yellow cotton wool on grass.	On yellow cotton wool on raised board.	On blue cotton wool on grass.	On blue cotton wool on raised board.	On white wadding on grass.	On black wadding on grass.	On flannel on grass.	On flannel on raised board.	On raw silk on grass.	On raw silk on raised board.	On silk from cocoon.	On the raised board.	On saw-dust on the raised board.	On black-lead on raised board.	On charcoal on raised board.	On lamp-black on raised board.	On whiting on raised board.	On chalk on raised board.	On tinfoil.	On lead.	On pantile.	On slate.	One-fourth of an inch above water.	On paper on raised board.	Modification.	Amount 0–10.	High or low.	Direction.	Strength 0–6.	Haze, fog, mist, or vapour.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°						
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Cirrostratus	10				
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...	...	...	...	...	...	...	...	...	...	...	...	...	...	−1·3	−3·0	...	...	...	...	...	...	...	...	...	Cirrostratus	10				
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...	...	...	...	...	...	...	...	...	...	...	...	...	...	−0·9	−1·3	...	...	...	...	...	...	...	...	...	Cirrostratus	10				
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·8	...	1·3	...	0·4	...	...	...	1·5	...	1·0	...	...	1·4	1·4	1·2	...	1·8	...	...	...	...	...	1·0	...	Cirrostratus	10				
·2	...	3·4	...	3·1	...	...	...	3·0	3·1	...	...	1·1	...	3·1	2·6	...	2·8	3·8	...	...	0·5	1·5	1·9	...	Cirrostratus	10	H.			
·3	...	1·0	...	0·5	...	...	4·3	...	3·0	...	...	...	...	1·5	0·5	...	0·3	0·5	−0·3	1·0	0·5	0·5	−0·5	...	Cirrostratus	10	H.			
·5	1·5	...	1·5	...	1·3	...	1·5	...	2·0	...	...	...	...	−0·3	1·0	...	...	...	...	...	...	...	0·2	...	Cirrostratus	10				
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3·8	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Cirrostratus	10	H.			
4·5	...	2·6	...	3·0	...	...	...	2·4	...	...	...	...	...	2·9	2·3	...	3·6	...	...	...	...	...	1·6	...	Cirrostratus	10				
6·1	...	4·1	...	4·1	...	...	...	4·1	4·1	...	...	2·1	...	...	...	...	...	2·6	...	...	...	...	3·1	...	Cirrostratus	10	H.			
8·1	5·1	...	3·1	...	...	...	4·0	...	3·9	...	...	...	...	...	...	...	...	...	...	2·7	2·0	2·0	...	...	Clear, nearly	1	...	S.	2	
4·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Cirrostratus	10				
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7·5	5·5	...	6·3	...	...	...	5·7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear					

ica when abundant in granite; every fibre of flax was encrusted with ice, mixed with a few small flakes: raw silk was covered with spikes one-sixteenth of an inch length, two of which emanated from the same po'nt, and whose angle of separation was 30°: on blue and yellow wool there were many perpendicular spikes one-urth of an inch in length: flannel was covered with spikes one-eighth of an inch in length, all of which were inclined to the horizon at an angle of 60°: saw-dust as very beautifully covered with spiculæ fans, formed by several spikes emanating from the same point at all angles greater than 30° with the horizon; these pikes were connected with each other by lateral spikes or bands; the whole forming the appearance of a fan, whose extent was 120°.

Sept. 23, 5^h 15^m. The observations taken at this time have been inadvertently omitted in subsequent calculations.

In the column whose heading is "Direction of Wind," N. denotes north; E. east; S. south; W. west; C. calm, and G. gusts.

Table (Continued).

Number of the Tables.	1843. Astronomical day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air above that placed																					
			On long grass.	On short grass.	On raw wool on grass.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On garden mould.	On gravel.	On river sand.	On river sand on the raised board.	One inch high above grass.	Two inches high above grass.	Three inches high above grass.	Six inches high above grass.	Twelve inches high above grass.	On unwrought white cotton wool on grass.	On unwrought white cotton wool on raised board.	On raw wool on raised board.	On flax on raised
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
VII. (Continued.)	Oct. 25. 21 0	34·1	5·1	4·1	6·1	6·1	4·1	2·1	−7·9	−5·4	...	6·1	4·1	2·1	...	3·1	...	...	1·9	1·9	6·1	2·6	...	
	26. 17 30	33·1	5·8	2·6	5·4	5·4	4·5	1·2	−7·7	−5·0	...	2·4	3·2	2·0	...	2·3	...	...	1·1	0·0	5·6	...	...	..
	Nov. 3. 5 30	49·3	5·3	5·3	7·5	8·1	6·3	2·0	...	...	...	...	1·5	2·5	...	3·7	...	...	1·4	0·7	7·1	...	...	..
	7. 10 0	42·5	5·0	5·3	8 5	6·5	5·7	2·2	−4·5	0·0	...	3·0	2·7	3·0	...	4·5	...	...	0·3	−0·2	7·0	...	...	5·
	7. 11 10	43·5	5·5	6 3	9·5	6·5	6·5	2·1	−1·7	2·5	...	3·5	2·5	2·5	...	4·5	...	...	0·5	−0·3	8·5	...	...	4·
	10. 17 30	34·7	5·7	5·5	6·6	5·5	7·7	3·3	−4·3	1·7	...	3·7	1·7	2·5	...	...	...	...	1 5	0·0	6·1	...	...	..
	12. 13 5	33·0	5·7	5·7	7·7	6·7	4·2	3·7	−3·9	−0·3	...	...	...	...	...	...	...	...	0·7	0·5	5·7	...	...	..
	13. 10 10	34·5	5·5	4·5	4·0	3·5	4·5	2·5	−2·5	2·5	...	...	...	...	...	...	...	...	1·3	1·3	4·0	...	...	..
	15. 4 15	38·0	5·9	5·0	6·5	5·7	7·0	2·5	−2·7	0·7	...	...	...	...	...	...	...	...	3 2	0·8	5·7	...	...	..
VIII.	Sept. 16. 11 20	58·7	6·0	3·7	...	...	4·7	...	...	...	...	4·2	2 7	3·4	3·5	3·5	...	1·2	...	...	...	...	...	..
	17. 12 30	61·9	6·9	3·5	...	...	5·0	...	...	...	...	4·1	2·2	3·6	4·2	3·8	...	2·5	...	...	...	...	...	..
	17. 13 20	60·4	6·5	4·3	...	...	4·2	...	...	...	...	4·2	2·1	3·4	3·3	3·4	...	2·3	...	...	...	...	...	..
	17. 15 20	59·4	6·9	4·4	...	...	3·7	...	...	...	...	3·8	2·7	2·8	2·7	3·6	...	2·4	...	...	...	...	...	..
	20. 13 0	52·8	6·1	4·4	...	...	4·8	...	...	...	...	4·0	...	...	...	3·6	...	2·8	...	...	7·2	...	...	..
	20. 13 50	51·4	6·7	4·6	...	...	5·2	...	...	...	...	...	...	...	...	4·1	...	3·0	...	...	8·4	...	...	..
	Oct. 2. 5 0	61·0	6·0	5·0	...	...	5·0	...	...	...	...	...	...	...	...	4·5	...	...	3·5	...	...	...	...	..
	2. 11 15	53·3	6·3	5·8	...	...	6·0	3·6	...	...	...	...	...	3·3	...	5·5	...	...	3·1	...	3·9	...	...	..
	5. 9 0	53·9	6·3	6 9	...	...	5·4	2 3	...	...	...	...	...	2·7	...	4·7	...	...	2·9	...	...	...	...	..
	5. 10 40	54·0	6·0	4·2	...	...	4·5	2·3	...	...	...	...	...	3·2	...	3·8	...	...	2·6	...	...	...	...	..
	12. 14 0	35·5	6·5	6 0	11·5	7·5	5·5	3·3	...	...	...	...	...	...	...	4·5	...	...	2·5	...	8·0	...	...	..
	12. 18 0	38·0	6 0	6·0	...	...	5 0	2·5	...	...	...	3·2	3·2	3·0	...	5·8	...	...	2 0	...	...	...	...	..
	12. 18 15	38·5	6·5	6·5	9·0	9·5	5·5	2·5	...	...	...	...	...	...	...	4·5	...	...	3·7	...	9·7	...	...	..
	13. 8 0	41·2	6·7	6·2	11·2	9·2	6·2	3·2	...	...	...	...	...	...	...	4·2	...	...	2·2	...	9·2	...	...	..
	16. 9 20	33·7	6·5	4·5	8·7	10·1	7·0	2·2	...	...	...	...	...	...	...	2·7	...	...	1·9	...	8·1	...	...	..
	16. 11 0	32·8	6·8	5·8	9·3	9·3	7·3	3·0	...	...	...	...	...	...	...	4·3	...	...	2·3	...	7·8	...	...	.
	16. 12 10	34·4	6·4	5·4	6·4	6·4	6·7	2·4	...	...	...	...	...	...	...	3·4	...	...	1·6	...	6·4	...	...	..
	16. 14 0	38·0	6·0	3·0	6·0	6·0	2·0	0·5	...	...	...	...	...	...	...	2·0	...	...	1·0	...	6·0	...	...	.
	17. 7 0	39·3	6·8	4·5	9·8	9·3	5·3	2·8	...	...	...	2·8	2·3	2·3	...	3·8	...	...	2·3	1·3	9·3	...	...	.
	18. 17 0	28·5	6·5	6·3	10·5	8·5	9·7	4·7	...	...	...	3·9	0·8	3·0	...	6·1	...	...	3·5	0·3	10·5	...	...	..
	19. 15 30	29·6	6·5	4·6	10·8	10·6	7·1	6·6	...	...	...	4·8	3·6	4·4	...	4·6	...	...	0·6	0·6	10·6	...	...	.
	19. 17 30	29·6	6·6	6·1	9·0	10·6	4·7	3·9	...	...	...	1·8	3·6	5·8	...	5·6	...	...	2·0	1·8	9·0	...	...	.
	19. 19 30	32·5	6·3	5·7	5·7	5·9	5·0	3·3	...	...	...	3·3	1·5	3·5	...	5·5	...	...	2·7	1·5	6 3	...	...	.
	21. 4 25	49·5	6·5	4·3	7·0	6·5	5·5	1·5	−0·5	0·0	...	1·3	2·5	2·7	...	4·0	...	...	2·5	2·0	6·0	...	...	.
	21. 11 30	39·0	6·7	4·5	9·0	8·0	5·3	2·6	−4·0	−1·3	...	3·0	2·0	2·4	...	2·8	...	...	0·6	0·2	6·5	...	...	.
	23. 5 0	50·3	6·5	4·1	8·0	6·5	4·9	1·4	−0·5	0·5	...	2·3	1·0	2·6	...	3·0	...	...	0·1	−0·3	7·1	...	...	.
	26. 5 10	43·5	6·7	2·8	5·0	5·0	3·1	2·0	−4·3	−2·4	...	1·7	0·3	1·3	...	1·7	...	...	−0·7	−0·9	5·0	...	...	.
	26. 15 30	33·1	6·3	3·5	6·4	6·7	4·1	2·6	−7·7	−5·0	...	2·6	4·3	1·7	...	3·8	...	...	1·1	0·0	6·3	...	...	.
	26. 19 0	33·3	6·3	4·3	6·7	7·1	3·0	2·5	−6·7	−8·0	...	2·3	4·3	3·5	...	4·7	...	...	1·3	1·3	5·3	...	...	.
	Nov. 8. 9 10	37·8	6·7	6·7	7·8	8·0	7·8	4·3	−3·4	4·8	...	4·1	3·8	4·1	...	5·8	...	...	2·6	1·8	8·6	...	...	5
	8. 11 0	36·6	6·6	6·6	8·6	8·6	7·1	3·6	−4·5	4·6	...	4·6	3·6	4·6	...	6·6	...	...	1·8	1·6	8·6	...	...	(
	8. 12 40	41·4	6·4	5·9	7·9	5·9	5·9	1·4	−1·6	1·4	...	1·6	0·9	1·9	...	...	...	...	0·2	0·2	7·4	...	...	4
IX.	Sept. 16. 9 10	61·5	7·6	8·3	...	...	5·5	...	...	...	...	6·0	0·7	3·6	4·5	7·5	...	3·7	...	...	...	...	...	.
	17. 11 0	62·3	7·0	6·0	...	...	5·3	...	...	...	...	3·8	1·5	3·3	3·8	4·3	...	2·7	...	...	...	...	...	.
	17. 11 30	62·4	7·6	6·6	...	...	5·6	...	...	...	...	4·5	2·0	4·2	4·3	4·6	...	2·7	...	...	...	...	...	.
	18. 7 30	66·1	7·8	6·3	...	...	6·6	...	...	...	...	5·1	1·8	4·0	3·8	5·5	...	3·4	...	...	...	...	...	.
	20. 10 50	54·5	7·5	5·5	...	...	4·5	...	...	...	...	...	...	3·5	...	7·0	...	3·0	...	...	7·5	...	...	.
	20. 11 20	54·2	7·7	6·0	...	...	6·2	...	...	...	...	4·2	...	...	...	4·2	...	3·0	...	...	8·7	...	...	.
	20. 14 30	51·7	7·3	5·0	...	...	4·7	...	...	...	...	...	...	3·4	...	4·3	...	2·9	...	...	8·7	...	...	.
	24. 17 35	47·5	7·1	7·7	...	...	8 4	2·5	...	...	...	...	2·8	...	...	5·0	...	3·2	...	...	7·5	...	...	.
	27. 18 0	40·4	7·9	6·6	...	...	6 0	4·4	...	...	...	...	5·4	...	...	5·9	...	4·4	...	...	9·9	...	...	.
	27. 18 22	39·7	7·5	7·4	...	...	7·9	3·2	...	...	...	...	...	4·9	...	4·7	...	3·7	...	...	8·2	...	...	.
	27. 19 10	41·0	7·0	6·2	...	...	6·5	...	...	...	...	...	...	4·5	...	5·0	...	3·5	...	...	9·0	...	...	.
	Oct. 5. 5 20	60·3	7·3	6·1	...	...	5·8	0·8	...	...	...	...	−0·2	...	...	3·7	...	...	2·9	...	...	...	...	

Oct. 25, 21h 0m; the ice in evaporator is 0·5 in thickness three hours after sun-rise. Nov. 3, 5h 30m; scud and vapour. Nov. 7, 11h 10m; dew abund. Sept. 16, 11h 20m; a great deposition of moisture. Sept. 17, 12h 30m; the stars nearly obscured. Sept. 17, 13h 20m; a few stars visible in the zenith. S 20, 13h 0m; dew. Sept. 20, 13h 50m; dew. Oct. 2, 5h 0m; zenith clear. Oct. 12, 18h 15m; zenith clear. Oct. 17, 7h 0m; shortly after rain had fal Nov. 8, 9h 10m; no dew. Sept. 16, 9h 10m; the stars look small. Sept. 17, 11h 0m; zenith clear. Sept. 20, 11h 20m; dew. Sept. 20, 14h 30m; dew. S 27, 18h 0m; hoar-frost. Sept. 27, 18h 22m; hoar-frost; the sun up. Sept. 27, 19h 10m; hoar-frost disappearing.

Oct. 25, 20h 0m. On examining the several substances at this time, it was found that cotton wool was covered with spikes of ice one-sixteenth of an inch in th ness: on raw wool there were a few flakes of clear ice, and a few spikes; also each fibre was encrusted with small round particles of ice: on blue and ye wool the spikes were abundant, and all were inclined to the horizon at an angle of 30°: on every fibre of raw silk there were two spikes emanating from the s point and at right angles to the fibre, and inclined to the horizon at an angle of 30°: on black-lead, which substance was white with hoar-frost very early, t were clusters of spikes one-tenth of an inch in length, and inclined to the horizon at all angles: on charcoal there were clusters of spikes: on whiting there spikes at all angles: on flannel the spikes were one-eighth of an inch in length: on saw-dust the spikes were in bunches, many of which were of a fan-like sh and on wood there were many spikes.

Oct. 13, 20h 40m. After this observation, the three substances, raw wool, cotton wool and flax, were frequently examined, for the purpose of ascertaining time that each substance became free from dew; the sun was shining on all of them; it was noticed at 22h 30m that flax was free; at 14d 1h 30m cotton wool just free, but on raw wool small drops of water continued throughout the day.

TABLE (Continued).

Excess of the reading of the thermometer in air above that placed																									Clouds.			Wind.		
on grass.	On yellow cotton wool on grass.	On yellow cotton wool on raised board.	On blue cotton wool on grass.	On blue cotton wool on raised board.	On white wadding on grass.	On black wadding on grass.	On flannel on grass.	On flannel on raised board.	On raw silk on grass.	On raw silk on raised board.	On silk from cocoon.	On the raised board.	On saw-dust on the raised board.	On black-lead on raised board.	On charcoal on raised board.	On lamp-black on raised board.	On whiting on raised board.	On chalk on raised board.	On tinfoil.	On lead.	On pantile.	On slate.	One-fourth of an inch above water.	On paper on raised board.	Modification.	Amount 0–10.	High or low.	Direction.	Strength 0–6.	Haze, fog, mist or vapour.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°						
·6	...	4·3	...	4·3	...	...	...	6·6	...	4·3	...	3·3	3·0	4·3	3·6	...	4·3	...	...	...	...	...	...	...	Clear					
·3	5·2	4·6	...	4·1	...	...	...	4·0	4·4	...	...	4·8	2·8	5·6	5·4	...	3·4	...	...	...	...	...	2·1							
·3	...	5·2	...	...	...	...	...	3·5	...	4·1	...	3·3	...	4·1	3·9	...	4·1	4·7	...	...	...	...	2·1	...	....	2				
·2	4·7	...	5·3	...	5·7	...	5·0	...	6·0	...	...	...	...	...	...	...	...	...	3·3	5·0	3·7	4·3	2·3	...	Clear	...	...	S.	2	
·5	4·5	...	3·5	...	5·5	...	4·3	...	6·7	...	...	...	...	...	...	...	...	...	2·7	4·3	2·7	3·5	1·7	...	....	...	...	S.	2	
·5	...	...	...	...	...	...	...	...	...	...	...	...	...	2·7	2·7	...	...	...	...	...	2 6	3·3	2·7	...	Cirrostratus	7				
·7	...	...	...	...	2·7	3·7	4·7	...	4·9	...	...	...	...	2·7	1·7	2·7	4·7	3·2	0·7	1·7	2·7	2·5	...	...	Cirrostratus	10	H.			
·5	...	...	...	...	3·3	3·3	3·3	...	4·7	...	...	...	...	4·7	3·3	4·0	4·5	5·6	0·5	2·5	3·0	2·5	...	...	Cirrostratus	10	H.			
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)ct. 19, $19^h\ 30^m$. At 21^h the whole of the thermometers were read again; that in air had increased to 38°·2, and an increase from 10° to 20° had taken place in readings of the other thermometers.

)ct. 16, 11^h. The thermometer in the reflector is covered with dew: there is hoar-frost on long grass: on raw wool there are many square pieces of ice one-ntieth of an inch in thickness: at $11^h\ 25^m$ one piece of ice was removed from raw wool, which was three-fourths of an inch in length, half an inch in breadth, l one-sixteenth of an inch in thickness: on cotton wool the ice is forming in flakes, and there are some frozen dew drops on the grass.

)ct. 16, $14^h\ 50^m$. All the thermometers which were on substances, with frozen particles about them, continued to read 32° till this minute, and now they all d 39°; the increase of 7° must have been sudden. The circumstance of these readings continuing to read 32° for nearly three hours after the sky was cloudy, strates the effect of sensible heat becoming latent whilst the frozen particles were changing into a liquid state. At $17^h\ 20^m$ the clouds became broken and the was partially clear, rain having fallen within the interval; the reading of all the instruments decreased, but as the substances were all wet, it is probable that a part he decrease was owing to evaporation; no use was therefore made of these observations.

Sept. 20, $10^h\ 50^m$. At this time a sudden deposition of moisture took place whilst I was looking at the instruments, and they all became wet with dew, except t which was placed on sand. The readings of those instruments thus bedewed increased between 3° and 4°; the temperature of the air continued to decline as ore, so that the difference between the temperature of the air and those of the bedewed substances became less by the above amounts.

Table (Continued).

Number of the Tables.	1843. Astronomical day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air above that placed: On long grass.	On short grass.	On raw wool on grass.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On garden mould.	On gravel.	On river sand.	On river sand on the raised board.	One inch high above grass.	Two inches high above grass.	Three inches high above grass.	Six inches high above grass.	Twelve inches high above grass	On unwrought white cotton wool on grass.	On unwrought white cotton wool on raised board.	On raw wool on raised board.	On flax on raised board.
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
IX. (Continued.)	Oct. 5. 7 20	55·4	7·4	6·6	...	...	5·2	0·9	...	...	...	...	1·4	...	...	4·6	...	...	3·2	...	...	...	...	...
	9. 10 20	44·5	7·7	5·3	...	...	5·2	5·1	...	...	...	...	1·7	2·0	...	5·0	...	...	4·0	...	...	...	...	...
	9. 11 10	44·0	7·7	6·0	...	...	5·4	3·5	...	...	...	6·0	2·6	...	...	6·0	...	...	4·3	...	...	...	...	...
	12. 11 0	37·1	7·1	3·6	...	...	5·1	2·8	...	...	...	3·1	2·3	2·6	...	3·6	...	...	2·1	...	...	...	...	...
	12. 11 23	36·8	7·0	4·8	7·8	8·8	5·3	2·8	...	...	...	...	...	...	...	3·8	...	...	2·3	...	8·8	...	...	...
	12. 12 0	36·5	7·0	4·7	9·0	8·5	5·5	3·5	...	...	...	...	...	...	...	4·2	...	...	2·3	...	9·0	...	...	...
	13. 5 15	43·5	7·5	5 5	...	...	5·5	1·5	...	...	...	3·5	1·5	1·5	...	4·0	...	...	1·5	...	...	...	...	...
	13. 18 55	37·6	7·9	7·0	7·8	9·9	8·2	3·6	...	...	...	...	...	...	...	5·3	...	...	4·1	...	10·6	...	...	...
	14. 9 30	36·8	7·4	6·3	7·8	9·3	8·2	2·8	...	...	...	...	...	...	...	5·0	...	...	3·1	...	8·1	...	...	...
	16. 7 20	35·0	7·8	5·7	8·2	12 0	7·2	2·6	...	...	...	...	...	...	...	4·0	...	...	1·8	...	8·8	...	...	...
	16. 11 25	33·8	7·8	5·8	10 3	10·5	7·8	2 8	...	...	...	...	...	...	...	3·8	...	...	2·0	...	9·0	...	...	...
	16. 12 0	34·0	7·0	6·0	9·0	10·0	7·5	2·0	...	...	...	...	...	...	...	3·5	...	...	1·2	...	8·0	...	...	...
	20. 9 20	42·1	7·1	4·1	7·3	9·1	5·1	1·4	− 1·9	−0·1	3·1	2·3	2·4	2·9	...	3·1	...	...	...	...	9·1	...	...	...
	20. 11 0	42·0	7·0	4·5	10·8	10·0	7·0	2·0	− 1·0	0·0	3 0	3·0	3·0	3·3	...	3 0	...	...	1·0	0·2	6·8	...	...	...
	25. 19 30	31·0	7 0	5·0	9·0	8·7	5·0	3·0	−10·3	−6·3	4·2	1·4	−0 2	0·6	...	4·2	...	...	1·4	1·3	7·5	6·2	...	...
	26. 11 30	35·6	7·2	4·9	5·8	8·4	5·6	3·1	− 6·4	−4·4	5·3	1·4	5·8	2·8	...	5·3	...	...	1·4	0·9	6·2	...	...	...
	Nov. 7. 7 30	46·2	7·6	7·4	14·2	11·9	9·0	4·5	− 1·6	5·2	7·2	...	3·4	3·6	...	7·2	...	...	3·0	1·2	12·2	...	...	...
	11. 9 50	37·5	7·0	7·0	10·3	9·5	9·7	2·2	− 0·5	3·5	...	4·5	3·5	3·7	...	...	...	...	3·1	1·7	7·7	...	...	7·
	11. 12 0	35·5	7·5	6·5	10·9	9·5	7·8	3·5	− 1·9	1·5	...	3·5	2·5	4·2	...	...	...	...	1·4	0·5	7·5	...	...	7·
X.	Sept. 13. 15 20	49·5	8·7	8·5	...	...	11·0	...	...	...	...	...	1·5	7·0	5·0	8·5	4·5	...	...	...	9·5	8·3	...	...
	15. 9 10	61·5	8·5	7·0	...	...	6·0	...	...	...	...	5·0	1·9	3·5	5·3	5·5	...	2·5	...	...	8·5	...	...	...
	18. 8 0	65·3	8·0	5·9	...	...	6·2	...	...	...	...	3·5	5·3	3·8	...	6·1	...	3·1	...	...	...	...	...	...
	18. 11 20	60·3	8·0	6·1	...	...	8·6	...	...	...	...	...	0·1	...	...	7·6	...	4·1	...	...	9·4	...	...	...
	20. 8 45	57·6	8·6	8·6	...	...	8·8	...	...	...	...	5·8	2·8	...	...	7·8	...	4·3	...	...	10 8	...	...	...
	20. 9 55	57·3	8·5	8·8	...	...	6·1	...	...	...	...	4·3	1·3	4·1	3·9	5·3	...	3·3	...	...	...	...	...	...
	20. 11 40	54·2	8·4	7·5	...	...	6·2	...	...	...	...	...	2·0	...	...	4·2	...	3·0	...	...	8·7	...	...	...
	20. 15 20	52·0	8·5	7·6	...	...	6·0	...	...	...	...	...	2·7	...	...	6·4	...	4·0	...	...	9·2	...	...	...
	21. 18 45	51·8	8 3	7·4	...	...	5·8	...	...	...	...	...	...	4·0	...	6·0	...	3·1	...	...	8·1	...	...	...
	23. 5 40	62·6	8·1	6·6	...	...	6·2	1·6	...	...	...	...	...	2·6	...	4·4	...	3·4	...	...	9·4	...	...	...
	23. 7 30	58·3	8·3	6·8	...	...	7·3	2·3	...	...	...	...	1·1	...	...	8·1	...	3·7	...	...	9·5	...	...	...
	23. 11 35	52·1	8·2	7·1	...	...	5·6	2·1	...	...	...	6·6	...	...	...	7·1	...	3·9	...	...	7 2	...	...	...
	24. 13 30	52·5	8·5	7·1	...	...	6·8	3·5	...	...	...	5·5	...	...	...	5·0	...	3·8	...	...	8·0	...	...	...
	27. 11 30	41·2	8·7	7·4	...	...	8·2	4·2	...	...	...	7·2	...	...	...	6·2	...	4·0	...	...	11·4	...	...	...
	27. 15 15	39·8	8 8	7·3	...	...	7·3	3·8	...	...	...	...	...	6·0	...	5·0	...	3·8	...	...	10 3	...	...	...
	27. 17 15	39·5	8·0	8·0	...	...	7·5	3·9	...	...	...	5·0	...	...	...	5·5	...	4·5	...	...	10·0	...	...	...
	27. 18 48	40·4	8·4	8·4	...	...	8·6	...	...	...	...	...	...	4·9	...	5·4	...	3·4	...	...	10·9	...	...	...
	Oct. 2. 7 30	57·4	8·4	8·9	...	...	8·5	4·8	...	...	...	...	...	5·4	...	7·4	...	4·6	...	...	9·4	...	...	...
	4. 7 30	58·0	8·0	7·0	...	...	5·0	3·2	...	...	...	3·0	...	...	...	5·5	...	3·7	...	...	8·8	...	...	...
	9. 9 20	45·5	8·1	5·5	...	...	6·3	2·9	...	...	...	...	0·3	1·5	...	6 3	...	4 0	...	...	...	...	...	...
	9. 15 10	40·1	8·6	8·6	...	...	6·6	4·1	...	...	...	2·6	...	2·1	...	8·1	...	3·9	...	...	...	...	...	...
	12. 13 10	36·2	8·0	6·2	12·2	9·2	6·7	4·0	...	...	...	...	...	...	...	4·8	...	3 2	...	...	9·7	...	...	...
	13. 18 0	38·4	8·8	6 9	7·1	6·9	8·0	3·3	...	...	...	...	...	...	...	5·4	...	...	3·9	...	9·6	...	...	...
	13. 18 20	38·1	8·4	6·5	6·8	6·5	7·7	3·6	...	...	...	...	...	...	...	5·3	...	...	4·1	...	9·1	...	...	...
	13. 18 40	37·6	8·1	7·1	8·8	11·0	8 6	3·6	...	...	...	...	...	...	...	5·9	...	...	4·2	...	11·6	...	...	...
	13. 19 20	37·2	8·7	7·0	10·2	9·2	8·4	3·1	...	...	...	...	...	...	...	5·5	...	...	4·2	...	9·7	...	...	...
	14. 5 25	44·6	8·9	7·4	12·0	11·6	8·2	3·6	...	...	...	...	...	...	...	5·4	...	...	3·9	...	11·6	...	...	...
	14. 7 10	39·5	8·8	7·3	10·8	10·7	8·7	3·5	...	...	...	...	...	...	...	5·6	...	...	3·5	...	10·0	...	...	...
	15. 17 20	30·5	8·7	8·7	...	11·0	10·7	5·3	...	...	...	...	...	...	...	9·3	...	...	5·3	...	11·7	...	...	...
	20. 5 20	45·6	8·9	4·6	10·2	9·1	11·0	...	− 1·6	0·8	...	2·0	1·4	3·0	...	3·1	...	...	0·6	−0·2	7·4	...	...	...
	21. 5 0	48 2	8·2	5·5	9·2	8·9	5·2	2·1	− 1·0	0·0	...	2·2	2·2	3·2	...	4·5	...	...	2·5	2·1	8·7	...	...	...
	25. 15 0	32·9	8·1	7·5	11·0	11·5	7·9	4·4	− 9·6	−3·4	...	2·8	−0·1	2·4	...	...	...	...	2·2	1·3	8·9	...	...	...
	25. 18 15	30·3	8·0	6·7	10·6	11·1	6·8	3·9	−11·0	−6·7	...	2·5	0·3	1·3	...	...	...	...	1·9	1·4	9·0	8·8	...	...
	26. 9 0	38·3	8·3	7·1	10·0	9·8	7·3	3·0	− 4·7	−1·7	...	2·7	2·3	2·7	...	...	...	...	1·3	0·8	8·3	...	...	...
	Nov. 1. 8 30	38·0	8·0	6·0	10·5	10·5	6 0	3·0	− 6·0	−4·0	...	2·5	−1·0	2·0	...	...	...	...	2·0	1·5	10·0	...	...	...
	11. 5 30	40·5	8·3	5·4	8·5	8·3	6·0	2·5	− 2 3	−2·5	...	1·7	1·3	2·4	...	...	...	...	5·3	1·3	8·8	...	...	8
	12. 20 30	30·6	8·6	7·6	10·1	9·8	6·1	3·9	− 4·2	0·6	...	...	...	...	...	...	...	...	2·6	2·3	9·6	...	...	...
	13. 9 0	33·0	8·1	6·0	9·2	8·6	8·4	3·5	− 4·0	2·0	...	...	...	...	...	...	...	...	1·9	1·7	7·0	...	...	...

Oct. 14, $9^h\ 30^m$; the stars very dim. Sept. 15, $9^h\ 10^m$; horizon thick. Sept. 21, $18^h\ 45^m$; great deposition of moisture. Sept. 24, $13^h\ 30^m$; the stars app to be dim and small. Sept. 27, $15^h\ 15^m$; hoar-frost. Sept. 27, $17^h\ 15^m$; hoar-frost. Sept. 27, $18^h\ 48^m$; hoar-frost.

Oct. 12, $11^h\ 0^m$. The thermometer placed on long grass was thickly encrusted with hoar frost. At the time of the deposition of dew the readings of all thermometers increased which became wetted by it; that is, of every instrument whose reading was less than the temperature of the dew-point. The first appe ance of dew was in very minute drops on the pointed ends of long grass, which on receiving an increase of water flowed down the blades.

Oct. 21, $5^h\ 0^m$. At this time 10 grains of wool were placed on grass, and a similar parcel was placed on the raised board; at $7^h\ 30^m$ the former was found weigh 12·1 grains, and the latter 10·7 grains; the glass stems of all the thermometers were wet with dew, though the substances upon which they were placed w not yet wet; at 19^h the weight of the parcel which was placed on grass was 17 grains, that placed on the raised board had blown away.

Nov. 12, $20^h\ 0^m$. Wood was covered with spikes of ice one-fourth of an inch in thickness, inclined to the horizon at an angle of 60°, and at all azimuths;

Table (Continued).

Excess of the reading of the thermometer in air above that placed																									Clouds.			Wind.		
on grass.	On yellow cotton wool on grass.	On yellow cotton wool on raised board.	On blue cotton wool on grass.	On blue cotton wool on raised board.	On white wadding on grass.	On black wadding on grass.	On flannel on grass.	On flannel on raised board.	On raw silk on grass.	On raw silk on raised board.	On silk from cocoon.	On the raised board.	On saw-dust on the raised board.	On black-lead on raised board.	On charcoal on raised board.	On lamp-black on raised board.	On whiting on raised board.	On chalk on raised board.	On tinfoil.	On lead.	On pantile.	On slate.	One-fourth of an inch above water.	On paper on raised board.	Modification.	Amount 0–10.	High or low.	Direction.	Strength 0–6.	Haze, fog, mist, or vapour.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°						
.	...	...	...	...	...	...	...	...	...	...	...	...	...	5·5	5·9	...	...	...	...	...	...	...	...	...	Clear	...	...	C.		
.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5·2	...	...	...	...	...	...	...	...	...	Clear	...	...	C.	...	Mist
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rcoal and on black-lead there were spikes in rich clusters; those forming one cluster all emanated from the same point, and they were inclined at all angles; lampblack the spikes were very numerous, being about three-eighths of an inch in thickness, and inclined to the horizon at an angle of about 30°, and at all muths, so that each cluster formed a circle, in the centre of which there were no spikes: on sand there were spikes one-fourth of an inch in length, inclined at angles to the horizon: chalk was covered with a mass of spikes one-fourth of an inch in length: whiting was covered with rich bunches of spikes three-eighths an inch in length: flax was covered with small sparkling flakes of ice: raw wool was encrusted on each fibre so as to be about six times its own size: cotton ol was completely covered with spikes, and paper was covered with small round particles and a few spikes.

Oct. 13, $17^h\ 50^m$. At this time the sky was overcast and the readings of all the thermometers were nearly alike; at $18^h\ 0^m$ the instruments were read exhibiting above large differences.

TABLE (Continued).

Number of the Tables.	1843. Astronomical day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air above that placed																					
			On long grass.	On short grass.	On raw wool on grass.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On garden mould.	On gravel.	On river sand.	On river sand on the raised board.	One inch high above grass.	Two inches high above grass.	Three inches high above grass.	Six inches high above grass.	Twelve inches high above grass.	On unwrought white cotton wool on grass.	On unwrought white cotton wool on raised board.	On raw wool on raised board.	On flax on raised
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
XI.	Sept. 15. 7 20	63·2	9·9	8·4	...	...	7·7	...	...	...	...	5·2	1·2	3·2	2·2	6·2	...	3·2	...	...	9·2	...	...	...
	15. 11 0	58·8	9·9	8·8	...	...	7·8	...	...	...	...	6·8	3·6	5·3	6·8	7·0	...	3·6	...	...	10·8	...	...	...
	16. 7 40	65·0	9·1	9·0	...	...	7·3	...	...	...	...	6·5	0·3	4·0	3·4	7·0	...	4·2	...	...	...	...	...	...
	18. 9 30	63·0	9·5	9·0	...	...	7·5	...	...	...	...	6·0	3·0	5·3	...	7·2	...	4·5	...	...	10·0	...	...	...
	19. 15 10	53·0	9·3	7·8	...	...	11·0	...	...	...	...	...	3·5	...	...	7·5	...	3·1	...	...	8·7	...	...	...
	20. 10 25	55·5	9·5	9·0	...	...	8·0	...	...	...	...	...	3·0	...	...	7·0	...	4·0	...	...	10·5	...	...	...
	20. 15 0	52·6	9·7	8·3	...	...	6·1	...	...	...	...	5·1	...	...	...	6·9	...	4·6	...	...	10·7	...	...	...
	21. 6 55	62·7	9·7	9·2	...	...	9·2	...	...	...	...	...	1·7	...	...	7·0	...	4·5	...	...	8·2	...	...	...
	21. 7 30	61·5	9·5	8·5	...	...	7·3	...	...	...	...	6·5	...	...	...	7·8	...	4·5	...	...	10·5	...	...	...
	21. 18 20	51·3	9·0	9·0	...	...	8·0	...	...	...	...	...	...	5·9	...	7·5	...	3·5	...	...	10·0	...	...	...
	23. 6 0	61·4	9·7	7·4	...	...	7·4	2·4	...	...	...	5·0	...	...	...	6·4	...	4·1	...	...	11·4	...	...	...
	24. 15 10	50·5	9·0	7·0	...	...	7·9	3·0	...	...	...	...	...	5·5	...	6·0	...	4·1	...	...	10·5	...	...	...
	28. 11 35	45·3	9·5	6·3	...	...	7·3	2·8	...	...	...	...	...	3·9	...	4·8	...	3·4	...	...	10·0	...	...	...
	28. 13 25	43·5	9·6	8·0	...	...	11·3	3·0	...	...	...	7·7	...	...	...	5·5	...	3·8	...	...	12·5	...	...	...
	Oct. 15. 4 50	41·0	9·2	7·1	11·7	12·8	7·6	2·7	...	...	...	...	...	...	...	5·0	...	...	3·0	...	11·8	...	...	...
	15. 5 5	40·5	9·8	7·8	11·7	13·2	9·1	3·2	...	...	...	...	...	...	...	6·2	...	...	3·1	...	12·3	...	...	...
	18. 5 20	42·2	9·8	6·9	12·9	...	7·9	2·8	...	...	...	5·2	4·0	3·9	...	5·2	...	...	3·0	2·1	9·4	...	...	...
	18. 8 30	35·7	9·7	8·9	11·6	8·2	8·7	3·4	...	...	...	5·7	4·5	4·7	...	6·5	...	...	4·5	3·5	11·7	...	...	...
	19. 7 40	35·6	9·6	7·8	12·4	12·1	8·6	3·9	−6·9	−1·1	...	5·4	2·4	3·8	...	6·6	...	...	2·6	1·8	10·5	...	...	...
	19. 10 0	33·2	9·0	8·7	11·7	14·7	7·4	4·0	−6·8	−2·3	...	2·7	1·7	2·7	...	6·2	...	...	2·3	1·8	10·4	...	...	...
	19. 12 0	31·0	9·5	8·5	13·0	12·8	7·8	3·8	...	...	...	1·8	0·8	3·0	...	8·0	...	...	1·9	0·8	11·5	...	...	...
	21. 9 20	42·1	9·9	7·8	12·9	11·3	7·1	3·5	−1·1	−1·3	...	4·7	2·9	4·9	...	7·5	...	...	3·3	2·9	9·9	...	...	...
	25. 13 0	35·5	9·3	8·0	12·1	12·5	7·9	4·7	−7·5	−0·5	...	4·5	1·9	3·0	...	6·5	...	...	1·9	1·1	9·5	...	...	...
	26. 7 35	40·5	9·3	2·8	10·1	8·9	6·5	3·1	−3·5	−1·0	...	3·0	1·2	3·5	...	5·5	...	...	1·3	0·7	8·0	...	...	...
	Nov. 8. 12 0	35·5	9·5	8·3	12·0	11·8	9·3	4·3	−4·5	1·5	...	4·0	4·0	4·0	...	6·5	...	...	3·5	2·7	12·0	...	...	...
	8. 13 0	35·1	9·1	7·9	11·6	11·6	9·1	4·1	−4·1	1·1	...	4·1	4·1	3·6	...	5·6	...	...	3·0	2·3	11·6	...	...	...
	8. 17 0	33·5	9·0	8·0	11·2	9·5	8·5	3·5	−4·5	1·0	...	4·0	3·0	4·5	...	6·0	...	...	2·7	2·2	11·2	...	...	...
	9. 5 30	33·0	9·5	9·5	13·5	13·0	11·5	3·5	−6·0	−1·5	...	2·5	3·2	3·0	...	...	...	...	3·2	2·9	13·0	...	...	...
	10. 19 30	33·5	9·3	7·5	10·5	10·5	9·3	2·5	−4·3	−0·7	...	2·0	1·2	2·0	...	...	...	...	2·5	1·5	10·5	...	...	...
	11. 7 30	38·8	9·8	8·7	13·8	13·2	9·5	3·3	−1·6	1·8	...	6·0	4·2	4·8	...	...	...	...	3·8	2·6	13·7	...	...	10·[illegible]
	11. 10 35	37·0	9·2	9·5	10·8	10·5	8·8	3·8	−1·0	3·0	...	5·0	4·0	4·0	...	...	...	...	3·8	2·8	10·0	...	...	7·[illegible]
	12. 13 5	31·7	9·5	7·2	11·0	10·8	6·0	5·5	−6·2	0·0	...	...	...	...	...	...	...	...	1·0	1·0	7·2	...	...	...
XII.	Sept. 15. 10 33	59·5	10·0	7·9	...	...	8·0	...	...	...	...	5·5	2·3	5·0	...	6·5	...	3·5	...	...	12·0	...	...	...
	18. 9 0	64·0	10·0	9·0	...	...	7·0	...	...	...	...	8·0	3·0	8·0	...	7·0	...	5·5	...	...	10·0	...	...	...
	19. 10 0	59·2	10·7	9·5	...	...	9·2	...	...	...	...	...	4·2	...	...	9·2	...	4·7	...	...	11·2	...	...	...
	19. 11 10	55·5	10·5	10·3	...	...	10·3	...	...	...	...	...	2·5	...	...	9·0	...	5·3	...	...	11·1	...	...	...
	19. 13 10	53·7	10·7	7·7	...	...	11·7	...	...	...	...	...	3·7	...	...	7·1	...	4·4	...	...	6·7	...	...	...
	20. 9 25	57·7	10·2	9·2	...	...	9·2	4·2	...	...	...	...	...	3·7	...	7·7	...	4·2	...	...	10·6	...	...	...
	21. 6 10	63·6	10·1	7·4	...	...	...	...	...	...	...	...	...	4·5	...	5·3	...	4·3	...	...	9·9	...	...	...
	22. 5 40	62·6	10·1	9·2	...	...	7·2	...	...	...	...	4·1	...	...	...	5·9	...	3·8	...	...	12·3	...	...	...
	22. 7 20	58·3	10·6	10·5	...	...	9·3	...	...	...	...	...	0·6	...	...	8·1	...	4·5	...	...	12·7	...	...	...
	22. 8 10	56·2	10·5	10·3	...	...	7·4	...	...	...	...	...	...	5·0	...	7·2	...	4·3	...	...	11·2	...	...	...
	22. 11 30	52·0	10·9	12·0	...	...	9·5	3·6	...	...	...	...	...	...	...	7·8	...	4·8	...	...	11·5	...	...	...
	22. 13 30	50·7	10·0	8·5	...	...	8·7	3·6	...	...	...	...	2·2	...	...	7·7	...	3·5	...	...	9·9	...	...	...
	23. 8 30	56·5	10·5	9·3	...	...	7·5	3·0	...	...	...	...	...	6·5	...	8·5	...	4·1	...	...	11·5	...	...	...
	23. 10 50	52·8	10·8	8·3	...	...	7·8	2·8	...	...	...	...	...	7·1	...	8·8	...	4·8	...	...	12·3	...	...	...
	23. 11 10	52·2	10·7	9·1	...	...	7·2	2·7	...	...	...	7·5	...	...	...	8·6	...	4·6	...	...	12·2	...	...	...
	26. 11 30	41·4	10·1	10·0	...	...	9·1	3·9	...	...	...	...	...	3·9	...	8·6	...	4·2	...	...	11·4	...	...	...
	27. 9 10	41·5	10·7	9·3	...	...	10·5	5·2	...	...	...	9·5	...	...	...	5·8	...	5·5	...	...	11·5	...	...	...
	27. 11 0	41·5	10·0	7·7	...	...	8·0	4·3	...	...	...	...	...	6·7	...	7·0	...	4·3	...	...	11·5	...	...	...
	Oct. 2. 7 25	57·0	10·5	9·5	...	...	8·5	4·8	...	...	...	...	2·5	...	...	9·0	...	4·5	...	...	11·8	...	...	...
	15. 15 20	29·8	10·0	9·4	...	...	10·8	5·3	...	...	...	...	...	...	...	9·4	...	...	3·6	...	...	...	...	...
	18. 7 40	38·0	10·0	8·0	13·3	...	7·6	2·4	...	...	...	4·6	4·0	4·3	...	6·5	...	...	2·8	1·7	10·0	...	...	...
	18. 9 30	36·8	10·8	8·8	12·8	10·8	9·8	3·8	...	...	...	6·8	6·1	5·9	...	7·8	...	...	4·8	3·4	13·8	...	...	...
	18. 14 30	30·6	10·8	9·8	14·6	14·7	10·9	5·1	...	...	...	4·4	3·6	5·1	...	8·1	...	...	4·1	3·3	12·8	...	...	...
	18. 18 0	30·0	10·4	8·9	12·0	12·0	12·8	5·2	...	...	...	4·8	2·9	5·8	...	8·9	...	...	4·5	4·0	13·2	...	...	...
	19. 4 30	43·5	10·3	7·0	13·5	11·6	7·0	2·1	...	...	...	7·9	0·7	2·8	...	5·0	...	...	2·3	1·6	11·5	...	...	...
	19. 8 10	35·9	10·7	10·7	15·9	15·9	11·4	4·9	−6·1	0·9	...	5·1	3·7	6·1	...	9·1	...	...	3·9	3·2	13·7	...	...	...
	21. 7 30	44·0	10·0	7·8	12·5	12·5	7·5	3·5	−4·0	−0·5	...	4·5	2·8	4·0	...	6·0	...	...	2·0	2·5	10·2	...	...	...
	25. 9 30	38·2	10·4	8·2	13·2	13·2	7·2	4·2	−6·8	−0·2	...	3·7	1·0	0·7	...	7·7	...	...	1·2	0·7	13·0	11·7	...	...
	25. 11 40	36·6	10·4	8·4	12·6	12·8	8·4	3·6	−5·7	0·9	...	5·1	2·6	3·6	...	7·5	...	...	1·8	1·4	10·8	11·0	...	...
	Nov. 7. 5 30	51·7	10·5	9·7	13·7	11·4	9·7	4·2	2·6	5·7	...	4·1	4·1	5·4	...	7·6	...	...	4·5	1·7	11·7	...	...	...
	12. 19 20	33·0	10·0	7·9	10·4	11·0	7·4	5·8	−6·2	1·2	...	...	...	...	...	...	...	...	2·7	2·3	9·0	...	...	...

Sept. 15, 11h 0m; a thin cloud passing zenith. Sept. 18, 9h 30m; the stars seem small. Sept. 19, 15h 10m; the stars seem small, and they have a wateri appearance. Sept. 20, 15h 0m; deposition of moisture. Sept. 21, 6h 55m; the clouds are confined to the zenith and around it. Sept. 21, 7h 30m; the clou are confined to the zenith and around it. Sept. 21, 18h 20m; deposition of moisture. Sept. 24, 15h 10m; the stars are dim and small. Sept. 28, 11h 35m; a f clouds near S. horizon, and the stars are dim. Sept. 28, 13h 25m; stars seem small and shine dimly. Oct. 19, 7h 40m; dew just forming. Sept. 19, 10h 0 a few clouds. Sept. 19, 11h 10m; stars seem small and watery. Sept. 19, 13h 10m; stars seem small and watery. Sept. 23, 10h 50m; the south portion of s very clear. Sept. 23, 11h 10m; the stars have become dim. Oct. 2, 7h 25m; dew being deposited. Oct. 19, 8h 10m; dew. Oct. 21, 7h 30m; dew.

Oct. 19, 7h 40m. At 7h 0m a parcel of 10 grains of wool was placed on grass, and at this time it was found to weigh 11 grains. The temperature of the air 7h 40m was 3° less than it was 7h 0m, and yet the differences between the readings of the several thermometers and that in air, is less by 2° or 3° than they we

Table (Continued).

Excess of the reading of the thermometer in air above that placed																								Clouds.			Wind.		
On yellow cotton wool on grass.	On yellow cotton wool on raised board.	On blue cotton wool on grass.	On blue cotton wool on raised board.	On white wadding on grass.	On black wadding on grass.	On flannel on grass.	On flannel on raised board.	On raw silk on grass.	On raw silk on raised board.	On silk from cocoon.	On the raised board.	On saw-dust on the raised board.	On black-lead on raised board.	On charcoal on raised board.	On lamp-black on raised board.	On whiting on raised board.	On chalk on raised board.	On tinfoil.	On lead.	On pantile.	On slate.	One-fourth of an inch above water.	On paper on raised board.	Modification.	Amount 0–10.	High or low.	Direction.	Strength 0–6.	Haze, fog, mist, or vapour.
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e former time (see Table XIV.). It would seem from this circumstance that the heat evolved at the time of the deposit of dew was about 4°. At Oct. 19, 21^h ool was found to weigh 21 grains, and the thickness of ice in the evaporator was found to be 0·5 inch.

t. 15, $4^h\ 35^m$. At this time the thermometers were placed on their different substances, and large differences were almost immediately exhibited.

t. 19, $4^h\ 30^m$. These observations were taken two hours before sun-set; during the whole of this day the temperature of vegetation in the shade was much the temperature of the air; at $19^d\ 1^h$ the reading of a thermometer on grass was 39°·0, whilst that in air was 47°·2.

t. 25, $11^h\ 40^m$. The water which was placed in the evaporating dish exposed to the whole sky is frozen, whilst that placed in a similar dish exposed to one-f the sky only is not frozen.

Table (Continued).

Number of the Tables.	1843. Astronomical day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air above that placed																				
			On long grass.	On short grass.	On raw wool on grass.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On garden mould.	On gravel.	On river sand.	On river sand on the raised board.	One inch high above grass.	Two inches high above grass.	Three inches high above grass.	Six inches high above grass.	Twelve inches high above grass.	On unwrought white cotton wool on grass.	On unwrought white cotton wool on raised board.	On raw wool on raised board.
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
XIII.	Sept. 13. 15 15	49·8	11·3	10·3	...	...	11·8	...	...	...	...	...	2·8	8·8	8·3	10·3	5·3	...	...	...	12·3	...	...
	19. 7 20	62·4	11·4	10·4	...	...	...	...	...	...	...	7·4	6·3	6·4	...	8·2	...	6·0	...	...	11·7	...	...
	19. 9 20	58·7	11·2	9·2	...	...	9·2	...	...	...	...	6·7	3·2	5·7	...	8·7	...	3·7	...	...	10·5	...	...
	19. 9 40	59·3	11·3	9·8	...	...	9·3	...	...	...	...	7·3	4·3	6·3	...	9·3	...	4·8	...	...	12·3	...	...
	26. 9 10	47·2	11·7	10·7	...	...	10·7	6·2	...	...	...	...	...	5·7	...	7·2	...	5·7	...	...	13·7	...	...
	27. 6 40	44·5	11·7	8·3	...	...	10·5	5·3	...	...	...	...	...	3·7	...	9·3	...	5·4	...	...	12·7	...	...
	27. 7 30	44·0	11·7	8·7	...	...	10·1	5·0	...	...	...	...	2·7	...	...	7·8	...	7·8	...	...	13·0	...	...
	27. 13 0	40·3	11·1	9·1	...	...	8·8	3·8	...	...	...	...	4·8	...	...	8·1	...	4·3	...	...	13·3	...	...
	28. 17 25	38·1	11·7	10·1	...	...	10·1	4·6	...	...	...	...	...	7·3	...	8·1	...	5·2	...	...	12·6	...	...
	Oct. 18. 10 20	36·8	11·8	9·5	15·3	15·3	10·8	4·3	...	...	...	6·3	6·3	6·3	...	8·8	...	...	4·8	3·8	13·8	...	...
	18. 11 0	35·5	11·5	9·5	15·0	14·5	9·5	4·3	...	...	...	5·5	5·5	5·5	...	8·5	...	...	4·5	3·7	13·0	...	...
	18. 11 30	35·0	11·5	9·7	15·0	15·0	10·2	4·5	...	...	...	5·0	5·0	5·0	...	8·5	...	...	4·7	3·8	13·8	...	...
	19. 9 0	34·5	11·0	11·0	15·3	15·3	10·0	5·5	−5·5	−0·5	...	3·7	3·0	4·5	...	8·5	...	...	3·5	2·8	12·0	...	...
	25. 8 0	39·8	11·7	10·0	15·3	14·8	9·8	4·3	−5·7	−1·4	...	4·3	2·3	2·8	...	8·8	...	...	1·8	1·3	11·6	10·3	...
	26. 5 40	42·5	11·5	8·5	12·6	11·8	8·8	3·9	−3·5	−1·5	...	5·0	1·5	3·2	...	7·0	...	...	2·3	1·9	10·7	...	...
	29. 9 0	40·6	11·4	7·9	13·8	13·1	7·1	2·6	−2·4	−0·4	...	3·1	2·6	2·9	...	7·5	...	...	1·4	1·6	11·8	...	...
	Nov. 1. 4 30	43·0	11·1	9·0	14·5	13·5	8·5	5·0	−5·0	0·0	...	2·0	1·0	1·0	...	8·5	...	...	2·5	2·0	13·0	...	...
	1. 7 0	40·0	11·0	6·5	15·0	15·0	6·0	3·2	−5·0	−1·0	...	5·0	3·0	3·0	...	3·0	...	...	4·0	3·5	14·0	...	...
	4. 5 10	47·0	11·8	8·5	14·5	14·0	8·0	3·8	−3·0	1·5	...	5·0	0·0	2·8	...	7·5	...	...	2·2	1·8	12·8	...	...
	12. 6 0	37·5	11·0	11·5	13·7	13·5	9·5	4·0	−2·5	2·5	...	...	...	...	...	...	...	...	2·3	1·5	11·4	...	...
XIV.	Sept. 21. 8 30	41·0	12·2	9·5	...	...	7·0	...	...	...	...	4·8	...	...	...	8·0	...	4·5	...	...	14·8	...	...
	Oct. 16. 5 20	38·4	12·4	9·4	12·6	14·8	10·2	3·7	...	...	...	...	...	...	...	7·4	...	...	2·9	...	7·9	...	...
	18. 12 0	34·5	12·7	10·0	16·7	16·0	10·5	5·5	...	...	...	5·3	4·7	5·7	...	9·0	...	...	5·2	4·2	14·5	...	...
	19. 5 0	41·8	12·8	9·9	16·0	14·3	11·3	3·3	...	...	...	4·3	1·3	4·0	...	8·0	...	...	2·6	2·0	13·0	...	...
	19. 5 30	40·4	12·9	10·5	16·4	15·6	10·5	3·9	...	...	...	4·1	1·8	3·6	...	8·2	...	...	2·4	1·7	13·8	...	...
	Nov. 8. 19 0	31·3	12·3	9·3	16 3	13·3	11·3	5·8	−6·7	0·7	...	3·3	2·3	4·8	...	7·3	...	...	4·3	3·8	15·3	...	...
	12. 8 45	33·6	12·4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	12. 9 20	33·0	12·0	12·0	17·0	15·5	9·5	6·0	−5·0	4·0	...	...	...	...	...	...	...	...	2·5	2·2	12·0	...	...
	12. 10 10	32·3	12·5	12·1	17·3	15·1	10·3	6·3	−5·2	1·2	...	...	...	...	...	...	...	...	...	...	14·8	...	...
	13. 5 10	37·5	12·5	11·7	19·5	14·5	12·0	6·0	−1·8	1·7	...	...	...	...	...	...	...	...	...	...	14·0	...	...
	13. 5 30	37·6	12·6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	13. 7 0	36·0	12·5	11·0	11·4	12·0	11·0	5·4	−2·0	2·5	...	...	...	...	...	...	...	...	...	...	9·0	...	...
XV.	Sept. 13. 11 10	51·0	13·5	12·0	...	...	12·0	...	...	...	...	...	4·0	...	...	...	...	...	...	...	...	...	...
	13. 12 0	50·5	13·5	12·5	...	...	12·5	...	...	...	...	...	3·5	7·5	7·5	12·0	7·5	...	...	...	...	9·5	...
	13. 14 30	50·0	13·0	12·0	...	...	11·5	...	...	...	...	...	3·0	8·5	7·5	10·5	5·2	...	...	...	11·5	10·0	...
	28. 15 15	40·2	13·2	10·3	...	...	11·2	5·2	...	...	...	6·0	...	...	...	10·2	...	6·7	...	...	13·9	...	...
	Oct. 18. 13 0	33·4	13·2	10·4	15·9	15·9	10·4	...	...	...	...	4·1	3·4	5·2	...	8·6	...	...	4·0	3·1	14·1	...	...
	18. 16 0	30·0	13·2	9·7	14·0	13·2	8·2	4·5	...	...	...	4·0	3·5	3·8	...	8·9	...	...	6·0	5·0	12·8	...	...
	19. 5 50	40·3	13·9	12·1	16·3	16·3	11·6	4·5	...	...	...	6·7	3·2	6·3	...	9·9	...	...	4·7	3·7	15·0	...	...
	25. 5 15	45·5	13·0	10·3	16·5	16·1	11·5	10·5	−4·5	0·0	...	2·3	0·1	2·1	...	9·3	...	...	5·3	4·7	13·4	14·5	...
	Nov. 12. 8 30	33·6	13·1	12·4	16·8	16·1	10·6	5·6	−4·9	0·6	...	...	...	...	...	...	...	...	3·3	2·5	15·1	...	...
XVI.	Sept. 13. 12 30	50·8	14·6	13·3	...	...	13·3	...	...	...	...	...	3·8	7·8	8·3	12·3	6·8	...	...	...	...	11·6	...
	13. 13 30	50·7	14·2	12·7	...	...	13·2	...	...	...	...	...	3·7	9·2	8·9	11·7	5·7	...	...	...	11·7	11·8	...
	Oct. 19. 6 15	40·3	14·1	11·5	16·3	16·8	12·8	6·2	...	...	...	7·5	4 8	7·1	...	10·3	...	...	4·5	3·8	15·3	...	...

Sept. 13, 15^h 15^m; the sky is cloudy to 30° high. Sept. 19, 9^h 20^m; clouds all round to 10° high. Sept. 19, 9^h 40^m; a few clouds to the north. Nov. 13 5^h 10^m; dark clouds. Sept. 13, 11^h 10^m; great deposition of moisture. Sept. 13, 12^h 0^m; great deposition of moisture. Sept. 13, 14^h 30^m; great deposition o moisture. Sept. 28, 15^h 15^m; the stars small and dim. Sept. 13, 12^h 30^m; great deposition of moisture. Sept. 13, 13^h 30^m; great deposition of moisture.

Sept. 27, 13^h 0^m. Dew had been deposited previous to this observation: the thermometer placed on long grass was covered with hoar-frost, so much so that it stem had to be scraped before the reading could be taken: that placed on raw wool was similarly circumstanced, and it was firmly frozen to the wool.

Oct. 18, 12^h 0^m. The charcoal is white with hoar-frost: wool and flax are each a mass of ice, and the thermometers are firmly frozen to them. The thicknes of ice in the evaporator was 0·82 inch.

Table (Continued).

Excess of the reading of the thermometer in air above that placed																								Clouds.			Wind.		
On yellow cotton wool on grass.	On yellow cotton wool on raised board.	On blue cotton wool on grass.	On blue cotton wool on raised board.	On white wadding on grass.	On black wadding on grass.	On flannel on grass.	On flannel on raised board.	On raw silk on grass.	On raw silk on raised board.	On silk from cocoon.	On the raised board.	On saw-dust on the raised board.	On black-lead on raised board.	On charcoal on raised board.	On lamp-black on raised board.	On whiting on raised board.	On chalk on raised board.	On tinfoil.	On lead.	On pantile.	On slate.	One-fourth of an inch above water.	On paper on raised board.	Modification.	Amount 0–10.	High or low.	Direction.	Strength 0–6.	Haze, fog, mist, or vapour.
°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°						
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	....	3				
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.	...	Vap.
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	....	1				
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
...	...	...	...	...	...	...	...	...	...	...	...	...	8·1	8·7	...	...	...	...	...	...	...	...	...	Clear	...	...	c.	...	Vap.
...	...	...	...	...	...	...	...	...	...	...	...	...	8·1	8·5	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
...	...	...	...	...	...	...	...	...	...	...	...	...	7·7	9·0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.	...	Vap.
...	...	...	...	...	...	...	...	...	...	...	...	...	6·5	8·3	...	...	...	...	...	...	...	...	...	Clear					
...	...	...	...	...	...	...	...	...	...	...	...	...	7·7	9·2	...	...	...	...	...	...	...	...	...	Nearly clear					
...	11·0	...	11·1	...	...	...	8·8	...	10·8	11·6	8·8	6·8	7·6	8·8	...	7·8	...	...	...	...	...	...	...	Clear	...	...	...	...	Haze
...	11·0	...	11·5	...	...	...	8·5	...	12·0	12·0	9·5	5·5	7·5	8·6	...	7·5	...	...	...	...	...	...	...	Clear	...	...	...	...	Haze
...	10·5	...	12·7	...	...	...	9·5	...	12·5	11·5	9·2	6·5	9·0	8·8	...	9·2	...	...	...	...	...	...	...	Clear					
...	12·5	...	12·3	...	...	...	9·5	...	12·5	10·5	7·3	5·0	7·3	7·5	...	11·0	...	...	...	...	...	...	...	Clear	...	...	c.	...	Haze
...	11·6	...	11·6	...	...	...	11·3	...	10·7	...	7·8	6·8	9·0	7·9	...	8·8	...	...	...	...	...	4·8	...	Clear					
...	9·7	...	8·9	...	...	...	7·7	...	7·5	...	6·0	5·7	6·9	4·5	...	6·5	...	...	...	...	...	3·7	7·5	....	1				
...	4·6	...	4·6	...	...	...	2·6	...	5·6	...	5·0	...	...	3·8	...	5·4	...	...	...	...	...	2·4	...	Clear					
...	7·8	...	9·2	...	...	...	9·0	...	9·0	...	5·2	...	8·5	7·0	...	7·0	7·0	8·0	...	...	...	3·0	...	Clear					
...	5·0	...	5·0	...	...	...	6·0	...	6·0	...	6·0	...	6·2	6·0	...	7·5	7·5	7·0	...	...	...	5·0	...	Clear	...	...	c.	...	Fog.
...	6·5	...	8·0	...	...	8·6	6·5	12·5	...	...	9·5	...	6·2	5·8	...	6·4	6·0	...	...	5·5	5·5	2·8	...	Clear					
...	...	...	...	7·5	7·0	...	...	...	...	...	...	...	6·0	6·2	7·3	6·5	7·1	5·7	...	5·9	6·3	...	...	Clear					
...	...	...	...	...	...	...	...	...	...	...	...	...	3·0	3·5	...	...	...	...	...	...	...	...	...	Clear					
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
...	11·7	...	12·7	...	...	...	11·7	...	13·3	10·5	8·0	8·0	8·3	10·0	...	9·3	...	...	...	...	...	...	...	Clear	...	...	c.	...	Haze
...	9·6	...	9·6	...	...	...	9·6	...	12·6	8·6	7·0	6·6	7·0	7·6	...	7·8	...	...	...	...	...	...	...	Clear					
...	10·4	...	10·8	12·8	13·3	9·3	9·3	...	11·9	9·9	7·2	7·2	6·4	7·6	...	7·2	...	...	...	...	...	...	...	Clear	...	...	c.		
10·3	...	12·1	...	...	...	...	...	9·8	...	...	...	...	...	...	...	...	...	6·3	9·3	5·8	7·3	2·8	...	Clear	...	...	c.		
...	...	...	...	...	...	...	...	...	...	...	...	...	10·4	7·8	11·5	11·2	10·4	...	...	...	...	...	...	Clear	...	...	c.		
...	...	...	...	15·5	12·8	...	...	13·0	...	...	...	...	9·8	7·0	9·8	9·5	9·8	5·0	9·0	5·0	8·0	...	...	Clear	...	...	c.		
...	...	...	...	12·8	11·3	12·8	...	12·3	...	...	...	...	10·1	6·8	10·5	10·5	10·3	4·8	9·1	5·1	7·8	...	...	Clear	...	...	c.		
...	...	...	...	12·5	13·5	12·5	...	10·5	...	...	...	...	8·5	7·5	10·5	7·5	9·5	5·5	9·3	6·0	9·5	...	...	....	3				
...	...	...	...	...	...	...	...	...	...	...	...	...	7·6	7·1	9·4	7·1	8·7	...	...	...	...	...	...	Clear					
...	...	...	...	9·4	9·0	10·5	...	9·2	...	...	...	...	8·0	7·0	10·5	9·0	9·5	5·0	8·2	5·2	7·0	...	...	Clear					
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
...	...	...	...	...	...	...	...	...	...	...	8·4	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
...	...	...	...	...	...	...	...	...	...	...	7·2	...	...	...	...	...	...	...	...	...	...	...	...	....	1				
...	...	...	...	...	...	...	...	...	...	...	...	...	9·5	9·2	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
13·4	...	14·4	...	14·9	...	...	...	...	15·3	12·2	10·4	7·2	9·9	10·9	...	10·2	...	...	...	...	...	...	...	Clear	...	...	c.		
11·0	...	12·0	...	11·0	...	...	...	...	12·2	7·6	9·3	4·8	8·5	11·0	...	9·8	...	...	...	...	...	...	...	Clear	...	...	c.		
16·5	...	12·2	...	12·5	...	...	...	...	13·3	11·5	9·8	8·1	7·5	9·7	...	8·3	...	...	...	...	...	...	...	Clear	...	...	c.		
15·8	...	14·5	...	14·5	14·6	12·6	12·6	14·6	14·3	11·3	8·5	...	10·5	10·3	...	10·0	...	...	...	...	...	6·0	...	Clear	...	...	c.		
18·5	...	...	...	...	...	...	...	...	...	...	...	...	9·6	7·1	10·6	9·6	...	5·1	8·4	3·5	7·1	...	...	Clear	...	...	c.		
...	...	...	...	...	...	...	...	...	...	...	8·3	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
...	...	...	...	...	...	...	...	...	...	...	8·2	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		
16·5	...	13·8	...	14·1	...	...	...	...	16·1	12·1	10·1	9·3	10·3	11·5	...	10·1	...	...	...	...	...	...	...	Clear	...	...	c.		

v. 1, 4^h 25. An inch of rain had fallen previously, and at this time the sky became suddenly clear; dry substances were placed, and immediately the above differences were exhibited.

v. 12, 8^h 30^m. The reading of the thermometer on coarse flax was 18°·5 below that in air (see Table XV.); and Nov. 13, at 5^h 10^m, the difference between adings of the thermometer in air and that on raw wool was as large as 19°·5 (see Table XIV.). These differences were greater than any hitherto met with y person, and especial care was taken so as to be certain of their correctness.

Excess of the reading of a thermometer placed in air at the height of 4 feet, protected from the effec of radiation, above the readings of thermometers placed on different substances fully exposed to th

Number of the Tables.	1843. Day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air above that placed									On long grass covered by											On lambs' wool on grass.				
			On long grass.	On short grass.	On raw wool on grass.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	White raw wool.	Flax.	White tin.	White tin one inch high.	Blackened tin.	Glass.	Glass one inch high.	On garden mould.	Six inches high above grass.	One foot high above grass.	Two feet high above grass.	Jet black.	White.	Green.	Light blue.	Dark blue.
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
XVII.	Dec. 13. 17 15	41·5	0·5	0·5	1·5	1·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
XVIII.	Nov. 27. 19 30	48·3	2·8	2·7	3·5	2·2	2·9	0·3	1·6	2·1	...	...	...	...	...	...	...	...	1·6	...	−0·2	−0·4	...	...	...	...	...
	29. 21 0	35·0	2·5	3·8	2·0	1·5	2·0	−0·5	−5·0	−2·0	...	...	...	...	...	...	...	...	...	...	−0·2	−0·5	...	...	...	...	...
	Dec. 13. 17 0	41·5	2·5	2·5	2·5	2·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
XIX.	Nov. 20. 5 30	43·2	3·4	3·7	...	...	3·5	−0·8	−3·6	3·2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	20. 8 0	43·7	3·9	5·4	5·7	5·4	5·2	1·7	−0·5	3·7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	29. 18 0	33·7	3·9	3·9	5·7	5·7	3·9	1·7	−6·3	−2·3	...	...	...	...	...	...	...	...	...	...	0·5	0·2	...	...	...	...	...
XX.	Nov. 15. 5 10	37·0	4·0	4·0	4·4	4 0	4·2	1·8	−3·0	1·9	...	...	...	...	...	...	...	...	...	1·0	0·2	...	...	...	...	...	...
	27. 4 10	51·8	4·8	4·3	6·8	5·8	4·6	0·8	−2·8	3·0	0·0	...	...	...	...	...	...	...	3·3	...	−0·4	−0·4	3·4	2·9	3·3	3·0	3·0
	27. 11 30	48·5	4·7	4·5	5·9	4·2	4·7	1·3	1·0	1·5	...	...	...	...	...	...	...	...	3·5	...	−0·3	−0·6	...	...	...	...	...
	27. 13 30	47·0	4·0	4·0	5·5	3·4	4·7	0·6	0·6	1·0	...	...	...	...	...	...	...	...	3·0	...	−0·2	−0·5	...	...	...	...	...
	29. 19 0	34·5	4·7	4·7	6·7	5·7	4·0	2·4	−5·0	−1·0	...	...	...	...	...	...	...	...	...	...	1·4	0·7	...	...	...	...	...
	Dec. 6. 11 0	41·2	4·8	5·2	6·2	5·2	5·2	1·4	−0·8	3·2	−0·8	...	...	2·2	2·2	...	2·2	4·7	...	...	−0·2	0·0	...	...	...	...	...
	13. 18 0	40·0	4·5	4·5	4·5	4·5	3·0	1·8	−1·0	1·8	−0·5	1·5	1·5	2·5	2·5	4·5	5·0	2·5	...	...	−0·5	−0·5	...	...	...	...	...
	15. 3 45	48·7	4·7	4·7	3·7	4·6	4·5	0·6	2·7	3·4	3·3	...	...	...	...	3·4	3·9	1·5	...	...	−0·1	−0·3	...	...	...	...	...
	15. 19 30	43·0	4·0	5 0	4·0	5·0	5·0	1·2	1·2	3·0	−1·0	...	...	...	...	...	3·0	1·5	...	...	0·8	0·8	...	...	...	...	...
XXI.	Nov. 15. 4 15	38·0	5·9	5·0	6·5	5·7	7·0	2·5	−2·7	0·7	...	...	...	...	...	...	...	...	...	3·2	0·8	...	...	...	...	...	...
	18. 9 30	40·1	5·1	...	4·3	5·3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	18. 10 0	40·3	5·3	5·3	4·3	5·5	5·3	1·5	−0·7	2·1	...	...	...	...	...	...	...	...	...	0·3	0·1	...	...	...	...	...	...
	20. 9 30	42·0	5·5	4·5	5·2	6·0	5·6	1·0	...	3·7	...	...	...	...	...	...	...	...	...	−0·2	...	...	...	...	...	...	...
	23. 11 0	37·8	5·0	5·0	6·3	6·8	6·3	1·3	−3·2	−0·2	...	...	...	...	...	...	...	...	3·3	...	−0·2	−0·2	4·7	4·2	4·3	4·3	3·5
	28. 4 10	48·5	5·9	5·2	6·0	5·6	5·4	0·9	1·3	2·3	...	...	...	...	...	...	...	...	3·0	...	0·0	−0·3	1·9	2·0	2·4	3·1	2·6
	28. 5 0	47·4	5·2	4·5	5·4	4·3	4·7	0·7	0·7	1·8	...	...	...	...	...	...	...	...	2·8	...	−0·4	−0·6	3·1	3·3	2·8	3·3	3·1
	29. 17 0	33·8	5·8	4·3	6·6	5·8	3·8	0·8	−7·2	−2·2	...	...	...	...	...	...	...	...	...	...	−0·4	−0·9	...	...	...	...	...
	Dec. 5. 21 0	41·5	5·3	5·0	5·5	5·0	4·7	1·5	−0·7	1·5	0·5	...	...	3·8	3·7	...	2·7	2·3	...	...	0 3	0·0	...	...	...	...	...
	6. 3 45	46·2	5·8	6·2	6·9	6·1	5·5	1·2	0·4	3·0	2·1	...	...	3·2	3·5	...	4·4	3·2	...	...	−0·7	−0·5	...	...	...	...	...
	14. 3 0	45·7	5·9	5·2	5·2	4·5	6·7	0·5	1·7	3·7	2·9	3·7	3·7	2·9	2·9	4·9	4·5	2·5	...	...	−0·1	−0·3	...	...	...	...	...
	14. 4 0	44·7	5·7	5·8	5·8	5·5	5·7	1·2	1·7	2·9	2·5	3·7	3·7	4·2	2·5	4·7	3·8	1·7	...	...	−0·3	−0·3	...	...	...	...	...
	15. 5 0	47·6	5·6	5·3	5·4	5·9	5·6	1·4	2·6	4·1	2·6	...	...	...	...	...	4·2	...	...	...	−0·2	−0·4	...	...	...	...	...
	15. 18 0	43·2	5·4	5·2	4·6	4·7	4·4	1·4	1·2	2·9	1·2	...	...	...	...	...	3·6	...	...	...	−0·6	−0·8	...	...	...	...	...
	15. 21 0	45·0	5·0	4·5	4·3	5·8	5·0	1·8	2·7	3·0	2·2	2·3	...	...	...	...	3·0	...	...	...	−0·3	0·0	...	...	...	...	...
XXII.	Nov. 16. 13 5	31·0	6·3	6·0	5·8	6·2	4·5	2·8	−4·0	1·0	...	...	...	...	...	...	...	...	...	1·0	1·0	...	...	...	...	...	...
	18. 5 30	41·6	6·8	...	5·8	8·1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2·8	3·5	4·1	3·5	2·4
	23. 14 0	35·7	6·7	6·7	9·5	9·5	8·7	...	−3·8	−1·1	...	...	...	...	...	...	...	...	4·5	...	1·2	1·2	...	...	...	...	...
	23. 15 0	35·0	6·5	6·8	10·0	9·8	9·0	2·7	−4·0	−2·0	...	...	...	...	...	...	...	...	4·0	...	1·2	1·0	...	...	...	...	...
	24. 4 0	40·7	6·7	6·2	6·7	6·7	6·7	3·0	−2·3	−0·3	...	...	...	...	...	...	...	...	4·2	...	1·9	1·7	...	...	...	...	...
	28. 3 45	49·1	6·1	4·1	5·6	5·5	5·6	2·7	1·7	1·6	...	...	...	...	...	...	...	...	3·1	...	0·1	−0·1	...	...	...	...	...
	28. 21 0	44·8	6·6	7·0	7·8	6·6	6·3	0·7	1·8	3·6	...	...	...	...	...	...	...	...	4·8	...	1·1	0·8	...	...	...	...	...
	Dec. 5. 17 30	40·6	6·6	5·6	6·1	6·3	6·1	2·0	−2·6	−0·6	...	...	...	...	...	...	4·1	...	...	...	−0·1	−0·3	...	...	...	...	...
	5. 19 30	40·5	6·7	5·5	6·5	7·0	6·5	2·1	−2·2	0·9	...	...	...	...	...	...	4·2	...	...	...	0·0	−0·3	...	...	...	...	...
	6. 7 10	42·4	6·9	6·4	6·4	6·4	5·4	2·3	−0·6	1·6	0·4	...	...	...	...	...	3·8	...	...	...	−0·6	−1·0	...	...	...	...	...
	6. 14 0	41·0	6·0	6·0	5·7	5·0	6·0	1·4	−0·2	2·0	−0·5	...	...	2·0	...	...	4·8	4·0	...	...	−0·2	−0·2	...	...	...	...	...
	11. 6 30	39·0	6·8	6·0	7·0	7·2	5·0	1·8	−2·5	0·0	−2·5	4·0	5·0	3·5	3·0	5·5	4·4	3·0	...	...	1·2	2 0	...	...	...	...	...
	11. 9 30	36·6	6·4	5·8	6·1	7·6	6·1	2·3	−3·3	−0·1	−3·3	3·1	3·6	2·6	3·1	4·1	2·8	1·3	...	...	0·6	0·4	...	...	...	...	...
	Dec. 13. 17 5	41·5	6·5	5·5	6·5	6·5	...	2·3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	14. 5 0	44·4	6·2	5·9	5·2	5·2	5·4	1·2	1·9	3·2	2·2	3·4	3·4	2·4	2·4	4·4	4·6	1·9	...	...	−0·7	−0·6	...	...	...	...	...

Dec. 6, 11h 0m. Dew is abundant, yet the temperature of the dew-point is from 4° to 5° below that of the air, being about the same as that of the metals whi are thickly covered with dew (see Table XX.).

Dec. 13, 12h 0m. The sky was overcast with a high and very black cirrostratus. The temperature of the air was 45°; the readings of the thermometers at bo

y, arranged according to the difference of the readings of the one on Long Grass and that in Air, from
servations between 1843, Nov. 16, and the end of the year 1843.

…lambs' wool on grass.		Excess of the reading of the thermometer in air above that placed			On zinc on grass.			On tin.						On stone.										Clouds.			Wind.		
Crimson.	Scarlet.	On copper on grass.	On lead on grass.	On iron on grass.	Thick.	Of moderate thickness.	Thin.	White on grass.	White one inch high.	Blackened on grass.	On tin-foil.	On pantile.	On slate.	Firestone.	Purbeck.	Portland.	On brick.	On glass on grass.	On glass one inch high.	On unwrought white cotton wool on grass.	On the raised board.	One-fourth of an inch above water.	On coarse flax on grass.	Modification.	Amount 0–10.	High or low.	Direction.	Strength 0–6.	Haze, fog, mist, or vapour.
°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°						
…	……	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	Cirrostratus	10				
…	…	2·5	2·3	1·2	…	…	…	2·0	…	…	…	1·8	2·4	1·6	…	…	0·7	…	1·0	…	…	0·7	…	Cirrostratus	10	…	…	2	
…	…	0 0	2 0	−0·6	…	…	0·1	0·0	…	1·4	…	1·5	1·6	…	…	…	1·6												
…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	Cirrostratus	10				
…	…	4·2	…	2·5	…	…	…	2·5	2·7	4·5	…	…	…	1·7	…	…	1·7	3·9	3·8	…	…	…	…	Cirrostratus	10				
…	…	…	4·0	…	…	…	…	…	…	…	2·7	2·9	4·7	…	…	…	…	…	…	…	…	…	…	Cirrostratus	10				
…	…	…	5·7	…	3·7	3·9	2·5	…	…	…	…	2·5	3·9	…	…	…	…	…	…	…	…	1·5	…	Cirrostratus	10				
…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	4·0	…	…	…	Cirrostratus	10				
3·0	2·0	3·3	2·7	2·6	…	…	…	2 8	…	…	…	2·0	2·0	1·8	…	…	1·3	…	…	…	…	…	4·5	……………	1	…	W.	1	
…	…	3·5	2·3	2·9	…	…	…	4 5	…	…	…	2·8	3·0	1·9	…	…	1·7	…	…	…	…	2·8	…	Clear ……	…	…	W.	1	
…	…	3·0	3·2	2·5	…	…	…	3·5	…	…	…	2·7	3·2	1·5	…	…	1·0	…	…	…	…	0·7	…	Clear ……	…	…	W.	1	
…	…	3·3	2·1	4·0	4·5	4·7	4·0	3·3	4·0	5·0	…	2·0	2·2	2·5	…	…	3·0	5·7	6·0	…	…	1·0	…	Cirrostratus	10	…	W.	1	
…	…	4·7	4·5	3·7	4·2	3·2	3·2	5·0	3·5	4·7	…	4·0	4 0	4·7	…	…	3·0	2·7	3·0	…	…	…	…	Wh. fleecy cl	7	…	W.	L.	
…	…	4·0	4·4	3·8	3·2	3·5	3·5	3·0	3·0	4·5	…	3·7	5·2	3·0	2·5	2·0	…	4·5	3·0	…	…	1·7	…	Cirrostratus	10				
…	…	3·2	4·0	3·2	3·1	3·5	3·2	…	…	3·9	…	…	…	1·9	1·7	1·7	2·5	4·2	2·3	…	…	…	…	Clear					
…	…	…	2·6	…	…	4·2	2·2	…	…	3·0	…	…	…	…	…	…	…	2·6	1·4	…	…	…	…	Clear					
…	…	…	4·0	…	…	…	…	…	…	…	3·2	2·7	2·8	…	…	…	…	…	…	5·7	…	…	…	Cirrostratus	10				
…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	Clear ……	…	…	…	2	
…	…	…	3·5	…	…	…	…	…	…	…	…	3·1	3·8	…	…	…	…	…	…	…	3·5	…	2·5	Clear					
…	…	…	3·5	…	…	…	…	…	…	…	…	3·2	3·8																
3·7	3·4	3·3	2·8	3·8	…	…	…	…	…	…	…	2·8	3·8	3·0	…	…	2·8	…	…	…	…	1·8	…	……………	5	…	W.	L.	Vap.
2·3	2·3	4·3	2 8	3·3	…	…	…	3·3	…	…	…	3·2	3·4	2·3	…	…	1·9	…	…	…	…	1·6	…	Cirrus ……	5	…	W.	1	
2·3	3·3	3·2	2·2	2·4	…	…	…	2·9	…	…	…	2·6	2·7	2·1	…	…	1·6	…	…	…	…	1·9	…	Cirrus ……	5	…	W.	1	
…	…	4·3	4·0	2·6	3·8	3·9	1·8	2·6	2·3	8·3	…	2·8	3·3	1·8	…	…	1·8	4·6	2·8	…	…	1·0							
…	…	4·7	4·7	3·5	4·3	4·7	4·5	5·0	4·3	4·7	…	3·3	4·3	2·7	…	…	…	4·3	4·0	…	…	…	…	Clear					
…	…	4·4	3·4	3·5	4·2	4·1	4·6	4·4	4·2	4·8	…	3·2	3·2	3·0	…	…	2·9	5·1	3·2	…	…	2·2	…	Clear					
…	…	4:5	2·9	3·5	3·7	3·7	3·9	3·5	3·7	4·2	…	…	…	2·2	2·4	1·9	…	4·9	3·5	…	…	…	…	Thin clouds	8	H.			
…	…	4·9	1·4	3·5	4·5	4·5	4·5	3·5	2·8	4·2	…	…	…	2·5	2·6	2·5	…	4·9	3·2	…	…	…	…	……………	3	…	W.	½	
…	…	3·9	3·8	3·8	3·8	3·8	4·0	…	…	4·6	…	…	…	2·9	2·6	2·9	2·8	3·8	2·5	…	…	…	…	Clear					
…	…	3·9	…	3·9	…	…	…	…	…	3·4	…	…	…	2·0	…	…	…	4·1	…	…	…	…	…	Clear					
…	…	3·5	3·5	3·8	…	3·5	3·3	4·0	3·5	4·2	…	…	…	3·0	3 8	2·8	…	4·2	2·5	…	…	…	…	Clear					
…	…	…	3·8	…	…	…	…	…	…	…	…	2·3	3·8	…	…	…	…	…	…	6·0	…	…	6·0	……………	10				
3·3	3·0	…	…	…	…	…	…	…	…	…	…	…	…	…	. .	…	…	…	…	…	…	…	…	Nearly clear					
…	…	4·7	3·5	3·8	…	…	…	4·0	…	…	…	4·5	4·2	3·7	…	…	2·7	…	…	…	…	2·7	…	……………	2	…	C.	…	Vap.
…	…	4·5	3·3	3·8	…	…	…	3·1	…	…	…	3·5	4·0	3·8	…	…	2·9	…	…	…	…	2·0							
…	…	6·5	4·9	4·2	…	…	…	3·7	…	…	…	2·9	3·9	1·5	…	…	2·2	…	…	…	…	2·2	…	……………	5	…	S.E.	L.	
…	…	4·6	3·4	3·9	…	…	…	3·5	…	…	…	3·6	3·8	2·3	…	…	2·3	…	…	…	…	2·1	…	Cirrus ……	2				
…	…	5·0	5·6	4·6	…	…	…	6·0	…	…	…	4·7	5·6	4·8	…	…	1·8	…	…	…	…	1·8							
…	…	6·1	5·6	4·6	5·3	5·3	5·6	5·4	…	5·5	…	2·6	4·7	…	…	…	…	5·6	…	…	…	…	…	Clear					
…	…	6·3	5·6	4·7	5·8	5·5	5·8	5·5	…	5·5	…	2·4	4·7	…	…	…	…	5·5	…	…	…	…	…	Clear					
…	…	…	4·2	4·4	…	…	…	4·4	…	3·6	…	4·4	4·4	…	…	…	…	5·4	…	…	…	2·1	…	Clear					
…	…	5·0	4·5	4·2	4·8	4·5	5·0	4·0	3·5	4·8	…	4·0	4·0	3·8	…	…	3·8	4·5	3·0	…	…	1·5	…	Wh. fleecy cl	7	…	W.	L.	
…	…	4·8	4·2	4·0	4·8	4·8	4·8	3·8	4·0	5·8	…	…	…	3·0	4·0	3·0	3·8	5·5	5·0	…	…	1·0							
…	…	4·6	4·6	3·4	5·3	5 2	5·6	3·6	3·6	4·3	…	…	…	2·3	2·6	1·9	1·8	4·6	3·5	…	…	0·6							
…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	……………	1				
…	…	5·2	4·1	3·2	4·2	4·2	4·2	3·4	3·2	4·4	…	…	…	2·6	2·9	2·2	…	4·6	2·9	…	…	…	…	Clear ……		…	…	¾	

and two feet high were 45°; the readings of those placed on long grass, short grass, raw wool and flax were all 41°·5, and those on all metals read 43°; the
perature on the surface of the ground was 42°·5, and the air was in gentle motion from the west; yet there was a small portion of dew on most substances,
ch seems anomalous as the temperature of the dew-point was 40°.

Table (Continued).

Number of the Tables.	1843. Day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air above that placed																							
			On long grass.	On short grass.	On raw wool on grass.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On long grass covered by							On garden mould.	Six inches high above grass.	One foot high above grass.	Two feet high above grass.	On lambs' wool on grass.				
												White raw wool.	Flax.	White tin.	White tin one inch high.	Blackened tin.	Glass.	Glass one inch high.					Jet black.	White.	Green.	Light blue.	Dark blue.
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
XXIII.	Nov. 18. 7 30	39·8	7·3	6·8	8·8	8·8	7·0	1·8	−1·5	2·2	...	...	...	...	...	...	...	...	...	0·6	1·0	...	3·6	4·0	3·8	5·1	3·6
	23. 12 0	36·5	7·0	6·3	8·0	8·5	8·0	2·5	−3·5	−1·0	...	...	...	...	...	...	...	...	4·0	...	0·0	0·0	...	...	...	...	...
	29. 4 40	46·7	7·7	6·7	9·7	8·7	6·1	2·7	1·2	2·9	...	...	...	...	...	...	...	...	...	...	1·7	1·2	...	...	...	...	...
	29. 11 0	42·8	7·8	7·8	7·8	8·8	7·8	2·3	3·8	−0·2	...	...	...	...	...	...	...	...	...	...	1·3	1·3	...	...	...	...	...
	Dec. 2. 3 40	40·8	7·0	5·9	5·8	6·9	6·8	1·6	−4·0	3·7	2·4	...	...	2·5	2·3	...	3·4	2·8	...	...	0·3	0·2	...	...	...	...	...
	2. 4 30	39·5	7·9	6·5	9·0	9·3	6·7	2·0	−4·3	1·2	2·2	...	...	2·2	2·5	...	4·2	3·0	...	...	−0·3	−0·5	...	...	...	...	...
	6. 4 15	45·8	7·6	6·8	8·4	6·8	6·3	2·2	0·4	3·0	2·5	...	...	3·3	3·3	...	4·8	4·0	...	...	0·8	0·5	...	...	...	...	...
	7. 19 0	49·0	7·0	6·0	9·0	6·5	5·0	2·0	2·7	4·0	2·2	...	...	...	...	...	3·0	...	...	...	−0·3	−0·3	...	...	...	...	...
	7. 21 0	45·0	7·3	5·8	7·8	6·6	6·0	1·8	4·6	5·6	3·8	...	...	...	4·8	...	2·8	1·8	...	...	1·6	0·8	...	...	...	...	...
	8. 7 30	45·0	7·8	8·0	10·0	9·0	5·4	3·0	1·8	6·0	3·0	...	...	5·2	6·2	...	...	4·8	...	...	1·2	0·2	...	...	...	...	...
	15. 15 30	46·5	7·3	6·5	6·0	7·3	5·9	2·1	3·2	4·3	2·5	6·0	...	...	...	...	4·7	3·2	...	...	0·5	−0·3	...	...	...	...	...
	17. 4 0	46·0	7·0	7·0	7·9	6·0	6·5	1·0	1·2	4·2	3·2	7·9	3·0	6·0	5·8	6·0	4·2	4·0	...	...	−0·2	0·0	...	...	...	...	...
XXIV.	Nov. 16. 11 25	32·2	8·0	6·8	10·8	7·2	4·7	1·5	−3·2	2·4	...	...	...	...	...	...	...	...	...	0·6	0·2	...	5·2	7·2	7·2	6·2	7·2
	18. 4 30	42·5	8·3	7·3	9·5	8·5	10·5	2·9	−1·0	2·0	...	...	...	...	...	...	...	...	...	0·5	−0·3	...	4·0	5·0	3·4	5·0	4·7
	18. 5 0	41·5	8·5	7·5	8·8	9·5	7·5	2·0	−1·2	4·5	...	...	...	...	...	...	...	...	...	0·3	0·5	...	4·3	5·2	4·4	4·7	4·0
	28. 16 0	49·0	8·8	7·8	9·4	7·5	7·4	2·8	−3·7	4·0	...	...	...	...	...	...	...	...	5·5	...	2·0	1·0	...	...	...	...	...
	29. 4 15	46·6	8·6	8·6	9·6	9·8	7·6	3·6	0·5	3·6	...	...	...	...	...	...	...	...	...	...	2·1	1·6	...	...	...	...	...
	29. 5 10	45·3	8·3	7·1	8·3	6·8	7·0	2·3	−0·2	3·6	...	...	...	...	...	...	...	...	...	...	0·6	0·3	...	...	...	...	...
	29. 9 10	42·5	8·5	8·5	9·9	11·0	7·5	3·5	0·2	4·7	...	...	...	...	...	...	...	...	...	...	1·5	0·9	...	...	...	...	...
	Dec. 1. 9 5	42·3	8·1	7·3	5·3	7·2	8·3	4·3	...	1·3	...	...	...	...	...	...	...	...	...	...	...	1·1	...	...	...	...	...
	5. 16 15	42·7	8·0	7·7	7·7	7·7	7·3	3·7	−1·3	2·0	...	...	...	...	...	...	5·6	...	...	...	0·9	0·4	...	...	...	...	...
	6. 4 45	45·2	8·2	7·2	9·7	8·6	6·5	3·2	0·4	3·2	...	...	...	3·0	4·8	...	5·2	4·8	...	...	0·5	0·2	...	...	...	...	...
	6. 5 10	44·5	8·0	7·0	8·9	7·9	6·6	2·8	...	...	2·4	...	...	...	...	...	...	...	...	...	1·0	0·7	...	...	...	...	...
	8. 13 35	38·1	8·3	4·1	9·6	6·9	6·6	4·1	−2·5	2·3	2·1	...	...	...	...	...	...	...	...	...	...	2·1	...	...	...	...	...
	11. 3 55	42·8	8·9	7·8	9·7	10·2	8·8	...	−2·7	3·3	−0·4	...	...	...	...	...	...	...	...	...	1·0	0·8	...	...	...	...	...
	17. 13 35	38·6	8·4	6·3	6·6	6·4	4·6	2·0	−2·2	−0·4	2·9	...	...	...	1·6	5·1	...	...	...	...	...	1·4	...	...	...	...	...
XXV.	Nov. 29. 7 30	43·7	9·7	8·7	10·7	11·7	9·2	5·2	−0·3	3·7	...	...	...	...	...	...	...	...	...	...	2·5	2·2	...	...	...	...	...
	29. 13 45	41·2	9·5	8·2	9·0	6·7	8·2	3·7	−0·8	3·0	...	...	...	...	...	...	...	...	...	...	2·0	2·0	...	...	...	...	...
	Dec. 8. 5 0	46·3	9·3	9·3	13·5	13·3	7·3	3·3	1·3	4·3	1·5	...	...	...	...	...	...	5·3	...	...	1·5	1·1	...	...	...	...	...
	8. 12 15	40·5	9·3	6·0	9·5	8·7	7·0	3·3	−0·5	2·5	1·3	...	...	...	...	...	...	4·3	...	...	...	2·5	...	...	...	...	...
	11. 4 30	41·2	9·7	8·7	10·7	11·2	9·2	2·4	−2·8	0·7	−1·6	5·2	4·2	4·2	4·2	8·2	5·4	5·0	...	...	1·0	0·7	...	...	...	...	...
	11. 5 30	40·1	9·3	9·1	9·1	9·3	9·1	3·3	−2·2	0·4	−1·1	...	...	...	...	...	5·5	3·5	...	...	0·6	0·4	...	...	...	...	...
XXVI.	Nov. 16. 7 10	38·5	10·8	10·8	18·5	13·5	11·5	7·0	−0 5	3·5	...	...	...	...	...	...	...	...	6·5	5·4	4·7	...	11·0	13·5	12·3	13·7	12·5
	18. 4 35	42·5	10·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	9 5	11·5	10·6	11·6	12·0
	28. 18 50	45·5	10·5	9·3	10·5	7·7	8·7	3·8	1·5	6·5	...	...	...	...	...	...	...	...	...	...	1·9	1·2	...	...	...	...	...
	29. 12 30	41·5	10·3	7·6	9·9	10·0	9·5	3·5	−0·5	2·6	...	...	...	...	...	...	...	...	...	...	1·5	1·0	...	...	...	...	...
	29. 15 0	37·5	10·5	7·5	14·0	13·0	10·0	3·3	−3·5	2·0	...	...	...	...	...	...	...	...	...	...	1·5	1·2	...	...	...	...	...
	Dec. 8. 4 10	48·0	10·0	10·7	12·2	11·8	...	...	1·8	...	...	...	...	...	...	...	8·0	...	...	...	1·7	2·0	...	...	...	...	...
	8. 9 30	43·5	10·3	8·5	13·3	12·2	7·1	3·2	0·8	6·5	1·7	...	...	7·7	9·2	...	7·5	4·9	...	...	1·9	1·5	...	...	...	...	...
XXVII.	Nov. 16. 5 10	40·5	12·5	9·5	15·3	12·0	8·6	5·5	0·5	2·5	...	...	...	...	...	...	...	...	...	2·5	2·0	...	11·5	11·0	10·7	10·7	10·5
	16. 7 45	36·2	12·7	11·2	17·2	12·7	9·2	5·0	−1·3	1·7	...	...	...	...	...	...	...	...	...	3·1	2·4	...	10·2	11·5	10·2	10·7	9·0
	16. 8 45	34·8	12·8	9·8	16·8	12·0	8·8	5·6	−2·2	1·8	...	...	...	...	...	...	...	...	...	3·3	2·8	...	10·3	11·3	11·8	14·3	12·8
	Dec. 8. 4 30	47·6	12·1	11·4	13·6	11·6	8·6	4·3	2·6	2·6	...	...	...	6·6	8·6	8·6	...	...	...	...	0·9	1·6	...	...	...	...	...
XXVIII.	Nov. 16. 9 40	35·5	13·5	12·0	17·5	12·0	10·3	5·7	−1·0	2·5	...	...	...	...	...	...	...	...	...	4·3	3·7	...	13·7	13·5	14·0	15·0	14·5

Nov. 29, 13ʰ 45ᵐ; there is a small quantity of cloud over the zenith. Dec. 8, 5ʰ 0ᵐ; there are a few light clouds scattered about the sky. Dec. 8, 12ʰ 1
dew is abundant.

Table (Continued).

Excess of the reading of the thermometer in air above that placed

ambs' wool n grass. Crimson.	ambs' wool n grass. Scarlet.	On copper on grass.	On lead on grass.	On iron on grass.	On zinc on grass. Thick.	On zinc on grass. Of moderate thickness.	On zinc on grass. Thin.	On tin. White on grass.	On tin. White one inch high.	On tin. Blackened on grass.	On tin-foil.	On pantile.	On slate.	On stone. Firestone.	On stone. Purbeck.	On stone. Portland.	On brick.	On glass on grass.	On glass one inch high.	On unwrought white cotton wool on grass.	On the raised board.	One-fourth of an inch above water.	On coarse flax on grass.	Clouds. Modification.	Clouds. Amount 0—10.	Clouds. High or low.	Wind. Direction.	Wind. Strength 0—6.	Haze, fog, mist, or vapour.
°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°						
4·0	4·8	…	4·8	…	…	…	…	…	…	…	…	3·8	4·4	…	…	…	…	…	…	…	4·8	…	2·6	Clear					
…	…	4·3	3·0	4·7	…	…	…	…	…	…	…	3·7	4·3	2·7	…	…	2·7	…	…	…	…	2·5	…	....	4	…	w.	L.	Vap.
…	…	4·5	2·5	4·7	…	3·9	…	…	4·2	5·7	…	4·9	4·5	5·1	…	…	2·9	…	…	…	…	…	…	....	5				
…	…	…	5·3	5·6	4·0	…	…	…	4·6	6·7	…	5·8	6·3	5·3	…	…	4·6	…	5·8	…	…	…	…	....	3				
…	…	5·0	2·1	3·3	3·0	3·3	3·6	4·3	3·3	5·4	…	2·9	3·3	3·4	…	…	1·6	6·5	4·3	…	…	2·0	…	Light clouds	…	…	…	…	Haze.
…	…	5·3	3·3	3·7	4·0	4·5	4·7	4·0	2·9	5·5	…	3·7	3·3	3·5	…	…	2·4	5·5	4·0	…	…	2·3	…	Light clouds	…	…	…	…	Haze.
…	…	5·8	4·3	4·8	5·0	5·2	5·3	5·3	5·0	6·0	…	4·6	4·6	4·0	…	…	3·6	6·3	4·6	…	…	3·0	…	Clear	…	…	c.		
…	…	…	4·0	…	4·0	4·0	…	5·0	…	5·8	…	…	4·2	…	…	…	…	4·2	2·8	…	…	…	…	Clear	…	…	c.		
…	…	4·8	5·3	4·6	5·6	4·6	…	5·0	4·8	5·6	…	…	4·8	4·6	…	…	3·3	5·6	4·8	…	…	…	…	Clear	…	…	c.		
…	…	7·8	7·0	6·2	…	7·0	7·0	6·0	6·0	8·0	…	6·0	7·0	5·0	…	…	5·4	7·5	6·0	…	…	…	…	Clear	…	…	c.		
…	…	…	3·2	…	…	4·5	…	…	…	5·5	…	…	…	3·3	…	…	…	5·3	3·5	…	…	…	…	Clear	…	…	c.		
…	…	6·2	1·2	4·5	5·5	5·5	5·5	4·5	4·5	4·5	…	…	…	2·2	3·2	3·2	…	6·0	4·0	…	…	4·0	…	Clear	…	…	c.		
8·2	5·2	…	5·4	…	…	…	…	…	…	…	…	1·8	3·7	…	…	…	…	…	…	10·7	…	…	8·0	....	8				
4·0	6·3	…	6·0	…	…	…	…	…	…	…	…	6·0	4·5	…	…	…	…	…	…	…	…	…	…	Clear	…	…	…	2	
4·3	5·0	…	4·3	…	…	…	…	…	…	…	…	4·3	4·0	…	…	…	…	…	…	…	2·7	…	3·7	Light clouds	…	…	…	2	
…	…	7·5	6·7	5·7	…	…	…	5·8	…	…	…	2·9	6·0	4·2	…	…	4·0	…	…	…	…	4·0							
…	…	7·4	5·6	6·1	…	…	4·8	5·6	…	7·6	…	6·1	5·8	4·8	…	…	4 6	…	…	…	…	…	…	....	4				
…	…	4·6	4·3	4·8	3·7	…	…	6·3	…	6·3	…	4·5	5·1	3·7	…	…	3·7	…	1·0	…	…	…	…	....	7				
…	…	6·5	4·5	3·8	…	…	4·0	5·5	…	8·0	…	5·5	4·7	5·5	…	…	4·7	…	…	…	…	…	…	....	3				
…	…	4·8	3·7	…	…	4·5	…	4·3	…	6·1	…	4·3	6·3	…	…	…	…	5·8	4·8	…	…	…	…	....	5				
…	…	7·1	6·9	5·7	6·9	6·7	7·2	6·9	5·6	6·9	…	4·9	5·9	…	…	…	…	6·9	…	…	…	…	…	Clear					
…	…	5·9	5·2	5·0	5·9	5·7	5·9	5·6	…	6·2	…	4·6	4·7	3·7	…	…	3·4	6·4	5·3	…	…	2 9	…	Clear					
…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	Clear					
…	…	…	5·5	…	7·1	8·1	…	…	5·1	7·9	…	6·5	4·9	…	…	…	…	6·3	4·1	…	…	2·1							
…	…	6·8	4·8	5·8	6·8	6·8	7·0	5·7	…	8·6	…	…	…	1·8	…	…	…	8·0											
…	…	4·1	4·6	…	5·2	6·0	…	4·4	…	5·1	…	…	…	…	…	1·5													
…	…	7·5	5·9	6·4	…	…	…	…	6·7	8·7	…	6·7	6·9	4·7	…	…	5·2	…	…	…	…	…	…	Clear	…	…	…	1	
…	…	10·3	8·2	7·0	6·0	6·0	5·7	6·3	6·0	6·8	…	6·7	7·4	6·4	…	…	5·7	9·0	8·0	…	…	5·2	…	....	…	…	…	…	Vap.
…	…	9·5	8·3	6·3	9·3	9·1	…	7·1	7·3	9·3	…	6·1	6·3	3·5	…	…	…	11·3	5·7	…	…	4·3	…	....	…	…	…	…	H. V.
…	…	…	9·0	6·0	…	…	7·5	7·3	…	6·5	…	…	7·8	5·0	…	…	…	8·2	6·0	…	…	3·5	…	....	…	…	…	…	H. V.
…	…	8·7	6·2	5·7	7·2	7·2	7·2	5·2	6·2	9·2	…	…	…	2·2	2·4	2·7	3·2	8·4	6·4	…	…	3·4	…	Nearly clear	…	…	…	…	Mist.
…	…	6·1	6·0	5·4	6·1	6·0	6·4	5·1	5·1	7·1	…	…	…	2·5	2·9	3·1	3·3	7·1	5·1	…	…	2·3	…	....	…	…	…	…	Mist.
11·0	11·7	…	…	…	…	…	…	…	…	…	…	7·0	8·7	…	…	…	…	…	…	16·4	…	…	11·8	Clear	…	…	…	…	Fog.
11·3	11·5	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	…	Clear					
…	…	9·3	7·5	6·5	…	…	…	6·5	…	…	…	4·3	5·5	5·5	…	…	3·3	…	…	…	4·5	…	…	....	1				
…	…	8·7	9·5	6·5	5·0	4·3	6·0	5·0	5·1	7·3	…	8·0	6·5	6·0	…	…	5·5	8 7	7·2	…	5·0	…	…	A few clouds					
…	…	7·5	6·3	5·5	5·8	5·5	5·3	6·5	4·5	7·5	…	5·5	5·5	4·7	…	…	5·0	7·5	5·5	…	3·5	…	…	A few clouds	…	…	…	…	Vap.
…	…	8·0	7·5	6·4	…	8·0	8·5	6·7	…	8·0	…	6·0	6·0	…	…	…	4·0	8·2	…	…	…	…	…	Light clouds	…	…	…	…	H. V.
…	…	9·1	8·3	7·3	8·3	8·5	8·5	8·5	9·5	10·3	…	7·0	7·5	6·0	…	…	5·0	8·5	…	…	4·5	…	…	Light clouds	…	…	…	…	H. V.
10·0	11·5	…	6·5	…	…	…	…	…	…	…	…	5·7	7·3	…	…	…	…	…	…	8·7	…	…	11·0	Clear	1	…	c.		
10·2	12·2	…	8·7	…	…	…	…	…	…	…	…	4·4	7·2	…	…	…	…	…	…	16·2	…	…	12·0	Clear	…	…	c.	…	Fog.
12·3	12·8	…	8·6	…	…	…	…	…	…	…	…	4·3	7·8	…	…	…	…	…	…	14·8	…	…	12·0	Clear					
…	…	8·6	7·3	6·8	9·1	8·1	9·1	8·1	8·4	9·0	…	6·1	5·6	5·6	…	…	4·8	10·4	7·4	…	…	…	…	Clear	…	…	c.		
14·3	13·7	…	9·9	…	…	…	…	…	…	…	…	5·0	9·3	…	…	…	…	…	…	16·7	…	…	12·0	Clear					

In addition to the tabulated experiments, a variety of others were always in progress. I may mention the results of a few of these.

I placed a large sheet of pasteboard vertically on the grass plat, and laid a thermometer close to its lower edge on each side of it; the readings of both these thermometers thus placed were found to be identical and intermediate to that placed in air at the height of four feet and protected from the effects of radiation, and that placed on grass fully exposed to the sky; the same relation was found to exist in what azimuth soever the board was placed.

I then placed the pasteboard at an angle of 45° nearly with the horizon, and laid the thermometers as before. In this situation their readings were found to be intermediate as before, but that which was exposed to three-fourths of the sky, read most nearly to that on the grass fully exposed to the sky, and the one which was exposed to one-fourth part of the sky only, read most nearly to that in air. The amount of these differences of readings was as nearly as could be determined exactly proportional to the amount of the exposed sky.

Hence, as a general fact it may be considered, that whatever diminishes the view of the sky as seen from an exposed body, causes its temperature to decrease less than it would if the exposure to the sky be complete.

Various experiments were made to ascertain the effect of covering plants at night by matting, or other thin substances; and it was always found that when the protecting substances touched the plant, much heat was conducted away from it, and such plant was at a lower temperature than when the substance was merely interposed between it and the sky; the thinnest substance thus interposed, at any distance from the plant, was found to be effectual in preventing the loss of its heat by radiation. It was found, however, that when a plant was thus itself protected, but yet was exposed to any body which was exposed to the sky, more heat radiated from the former to the latter, than from the exposed body to the plant, and thus it lost some heat.

The several thermometers at the different distances from the earth were for some time read at short intervals during the night and day, and it was found that except after noon, the reading of the thermometer at twelve feet from the earth was very nearly identical with the true temperature of the air.

The bulb of the thermometer thus exposed to the full rays of the sun was situated nine inches west of the plank which carried it, and whose width was three inches; and the cause of its readings being about 1° too high during the afternoon, was owing to the heat reflected from the plank to it.

During the summer of 1844 and the year 1845, the reading of this thermometer was frequently examined, and found always thus to agree. Hence there is no doubt that if a thermometer be freely suspended in the air with its bulb at the height of thirteen feet above the soil, and far from any object to reflect heat to it, its readings will represent the true temperature of the air at the time, and much more truly than those of any one placed near the ground, or within a few feet of walls or buildings.

Excess of the reading of a thermometer placed in air at the height of 4 feet, protected from the effects of radiation, above the readings of thermometers placed on different substances fully exposed to the sky, arranged according to the difference of the readings of the one on Long Grass and that in Air, from observations taken between 1844 January 1 and 1844 May.

Excess of the reading of the thermometer in air above that placed

Number of the Tables.	1844. Day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	On long grass.	On short grass.	On raw wool.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On long grass covered by: White raw wool.	Flax.	White tin.	White tin one inch high.	Blackened tin.	Lead.	Lead six inches high.	Glass.	Glass one inch high.	Hare skin.	Rabbit skin.
	h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
XXIX.	Feb. 21. 11 0	34·1	0·8	0·8	...	...	...	...	...	−1·8	−1·4	...	...	...	...	...	...	...	...	...	...	...
XXX.	Apr. 25. 5 45	60·7	2·9	6·2	5·2	2·5	0·2	0·7	−0·3	...	0·7	...	...	...	...	...	...	...	...	...	...	...
XXXI.	Feb. 16. 3 0	45·3	3·8	0·2	3·3	−0·7	3·8	−2·4	4·5	2·1	4·8	3·3	3·8	3·3	3·3	3·3	3·3	1·6	−2·5	3·2	...	...
	Mar. 6. 19 0	32·7	3·1	2·2	2·2	2·7	3·9	1·1	...	...	−0·6	...	...	...	...	...	...	...	...	0·7	...	...
XXXII.	Jan. 6. 7 30	42·5	4·3	3·7	3·7	3·5	5·8	1·5	0·3	3·5	...	...	...	...	...	...	...	...	...	2·2	...	...
	Feb. 1. 3 30	35·2	4·3	3·4	2·7	4·3	6·2	1·2	−0·8	0·7	0·2	...	1·7	3·0	3·7	3·7	...	...	3·0	−0·1	...	...
	Mar. 1. 11 0	41·8	4·3	3·5	2·0	2·9	3·8	0·3	...	...	0·7	...	...	...	...	...	...	...	1·8	1·2	...	...
XXXIII.	Feb. 7. 19 0	32·3	5·1	4·9	6·1	5·2	7·3	2·1	8·1	0·2	1·7	...	...	...	2·2	...	...	...	...	...	...	...
	22. 18 45	24·5	5·8	4·4	5·9	6·0	9·5	3·1	−7·3	−6·2	−6·5	...	...	...	...	...	...	...	4·5	1·5	...	...
	28. 12 0	38·2	5·0	3·7	3·2	3·2	3·2	1·7	0·4	...	...	...	...	...	...	...	...	...	...	...	...	...
	Mar. 20. 20 10	31·3	5·9	3·5	2·8	5·3	...	...	...	...	...	...	...	...	...	...	...	...	2·1	1·3	...	...
	Apr. 11. 13 0	46·8	5·8	5·3	7·8	7·6	2·8	1·8	...	...	−0·9	1·3	3·5	...	...	...	...	...	...	...	...	...
	15. 9 0	48·2	5·2	6·2	8·2	8·2	5·0	1·0	...	...	...	...	...	...	...	...	...	...	...	1·9	...	3·4
	23. 8 55	53·3	5·7	7·1	10·3	7·8	6·5	1·6	1·1	3·3	1·3	...	...	...	...	...	...	...	...	...	...	...
XXXIV.	Jan. 18. 19 0	39·5	6·7	5·7	7·3	4·5	6·8	1·9	1·7	4·3	3·1	...	...	...	...	...	...	...	4·7	3·1	...	...
	Feb. 1. 9 0	28·2	6·7	4·7	7·2	5·7	9·2	3·2	−6·0	−4·2	−5·0	−4·2	−3·3	−2·3	−1·8	3·2	...	3·7	4·4	1·2	...	...
	20. 9 10	30·1	6·9	5·1	3·5	4·9	5·8	...	...	−5·6	−1·9	...	...	...	...	...	...	...	...	...	...	...
	22. 14 30	24·4	6·4	5·4	7·3	7·3	10·4	4·8	−8·1	−5·6	−6·8	...	...	...	...	...	...	...	4·8	2·8	...	...
	22. 16 0	24·3	6·2	5·1	7·3	7·1	10·3	4·4	−8·2	−5·7	−6·7	...	...	...	...	...	...	...	4·3	2·1	...	...
	22. 17 30	23·6	6·1	4·8	6·9	6·7	9·6	3·0	−8·6	−6·7	−7·4	...	...	...	...	...	...	...	3·8	1·6	...	...
	28. 18 30	33·0	6·0	5·0	5·0	5·2	4·0	2·0	...	...	2·5	...	...	...	...	...	...	...	2·0	0·8	...	...
	Apr. 1. 15 30	39·1	6·3	5·1	9·1	9·1	3·6	1·9	−4·4	0·1	−3·5	−6·4	−5·9	...	...	...	...	...	4·1	3·9	−4·9	−2·9
	1. 17 0	37·5	6·6	6·5	9·5	9·0	4·5	3·0	−5·5	1·5	1·5	−7·0	−7·5	...	...	...	...	...	3·3	3·0	−5·5	−5·5
	17. 17 0	44·2	6·2	5·7	11·2	12·0	3·7	1·7	−0·8	4·2	0·7	−3·3	−2·8	...	...	...	...	...	4·7	4·2	−0·8	−0·8
	25. 6 15	59·4	6·9	7·4	8·4	6·9	4·4	...	−0·6	3·4	−0·6	...	...	...	...	...	...	...	...	...	...	...
XXXV.	Jan. 6. 5 0	44·5	7·2	6·5	6·9	5·5	4·5	2·5	0·3	3·2	...	...	...	...	...	...	...	...	...	2·9	...	...
	10. 7 0	39·0	7·0	5·8	7·0	3·4	3·5	1·6	0·5	4·0	...	...	...	...	...	...	...	...	...	2·8	...	...
	22. 7 30	37·0	7·5	6·8	11·2	8·0	9·5	3·1	0·2	1·5	2·0	...	...	...	...	...	...	...	4·9	5·8	...	...
	Feb. 22. 6 45	30·6	7·8	6·9	8·8	8·6	11·6	3·6	−3·4	−2·4	−3·7	...	...	...	...	...	...	...	5·6	3·3	...	...
	22. 12 30	25·3	7·0	5·0	7·2	7·5	10·7	3·8	−7·4	−5·5	−6·9	...	...	...	...	...	...	...	3·8	2·2	...	...
	23. 13 0	25·1	7·1	5·8	8·0	7·8	11·3	3·9	−7·4	−4·8	−7·1	...	...	...	...	...	...	...	4·8	2·8	...	...
	28. 17 0	33·7	7·7	5·5	8·7	6·7	5·2	...	...	...	−3·3	...	...	...	...	...	...	...	0·7	0·5	...	...
	Mar. 12. 9 0	38·0	7·9	4·6	7·0	6·0	8·2	2·7	...	...	1·0	...	...	...	...	...	...	...	...	3·4	...	...
	Apr. 1. 14 30	40·2	7·5	5·0	9·9	10·0	4·2	1·7	−4·8	0·2	−2·2	−5·8	−5·3	...	...	...	−4·8	4·8	4·4	3·2	...	...
	5. 17 30	41·0	7·2	6·2	7·4	7·8	9·0	2·0	−1·3	...	−1·0	...	...	...	−3·3	...	−0·3	−0·3	3·0	...	...	...
	17. 16 0	44·7	7·2	6·7	14·2	14·7	3·7	1·7	−1·3	3·7	0·7	−3·3	...	...	...	...	...	...	6·7	5·7	...	...
	29. 9 0	42·2	7·0	6·2	12·2	11·6	7·2	2·7	−2·8	1·7	2·7	...	...	...	...	...	...	...	...	...	...	...
XXXVI.	Jan. 17. 5 0	41·2	8·7	7·2	6·4	6·2	9·4	2·4	2·2	4·4	4·7	...	4·2	4·9	...	5·7	...	...	5·7	4·0	...	...
	30. 4 30	46·2	8·2	7·0	7·7	6·4	7·6	1·2	5·0	4·7	3·7	...	...	...	...	...	...	...	5·4	4·3	...	...
	30. 5 30	43·6	8·6	6·3	9·6	5·6	6·5	1·3	3·5	5·6	2·1	...	...	...	...	...	...	...	5·8	3·6	...	...
	Feb. 1. 7 0	30·0	8·8	6·0	10·8	8·8	10·0	4·0	−5·0	−2·2	−3·0	2·2	−1·5	2·8	...	4·5	...	3·0	3·5	3·8	...	...
	16. 8 0	37·5	8·0	7·0	7·0	7·5	7·5	2·5	−1·0	1·7	1·0	0·3	1·0	2·3	...	2·0	4·0	3·5	6·3	3·7	...	...
	Mar. 6. 11 0	33·6	8·6	5·1	8·6	7·4	8·8	2·9	...	...	−1·2	...	...	...	...	...	...	...	...	2·4	...	...
	20. 9 0	32·5	8·5	6·0	8·5	8·5	7·0	3·6	...	1·5	−3·5	...	...	...	...	...	...	...	3·5	1·5	...	...
	20. 10 0	31·2	8·2	5·2	8·2	7·2	7·2	3·5	...	1·2	−4·0	−6·8	−5·8	...	...	...	...	...	6·7	4·2	...	...
	20. 11 0	31·0	8·2	5·5	9·0	9·0	7·0	3·8	...	0·5	−5·0	−5·5	−5·0	...	...	...	...	...	5·0	3·2	...	...
	20. 11 30	30·9	8·4	5·4	10·4	11·4	7·4	3·9	...	−0·1	−4·6	−6·5	−6·1	...	...	...	...	...	6·4	2·9	...	...
	20. 18 20	38·5	8·8	6·3	13·5	13·3	9·2	4·5	...	−0·3	−5·3	1·3	1·8	...	...	...	...	...	3·5	2·9	...	...
	Apr. 1. 18 30	40·0	8·6	7·0	8·5	8·5	5·0	3·5	−3·0	1·0	1·0	−3·0	−3·0	...	...	...	...	...	4·5	3·0	−3·0	−3·0
	2. 9 0	48·5	8·7	8·7	12·0	12·0	8·5	3·7	−1·5	5·5	1·5	...	...	...	...	...	...	...	...	...	...	...
	8. 13 30	42·1	8·6	9·3	14·3	12·8	6·5	2·4	0·0	2·5	−1·2	...	...	...	...	...	...	...	8·7	6·8	...	...
	8. 15 30	41·2	8·7	9·5	15·5	13·8	6·5	2·2	−0·9	2·1	1·2	...	...	...	...	...	...	...	7·8	6·8	...	...
	8. 17 30	39·4	8·6	9·6	15·7	15·8	8·4	2·4	−2·1	2·4	1·6	...	...	...	...	...	...	...	6·4	6·0	...	...
	17. 15 0	45·5	8·5	8·5	16·5	16·5	4·5	2·5	−1·5	3·5	0·5	−4·5	−4·5	...	...	...	...	...	8·0	7·0	−0·5	−0·5
	17. 18 30	47·5	8·0	6·3	10·5	8·0	6·5	...	...	...	...	...	...	...	...	...	...	...	5·5	3·0	...	...
	23. 7 25	56·0	8·8	7·0	12·0	10·7	6·3	1·7	1·5	4·7	2·2	...	...	...	...	...	...	...	4·0	6·0	...	...
	27. 7 3	48·5	8·5	8·5	13·5	12·3	5·5	2·7	−7·8	−1·5	−3·7	...	...	...	...	...	...	...	...	...	...	...

1844. Day, hour and minute.	Above grass in feet: 1.	2.	4.	6.	8.	10.	12.	On lead: On grass.	Six inches high.	One foot high.	Three feet high.	On zinc: On grass.	Six inches high.	One foot high.	Three feet high.	Four feet high.	On copper on grass.	On iron on grass.
h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Feb. 21. 11 0	...	0·1	0·2	0·3	0·0	0·3	0·2	...	...	...	...	0·0	...	...	...	...	...	...
Apr. 25. 5 45	−1·4	0·7	0·7	...	0·9	...	1·0	3·2	...	...	...	0·7	−1·3	...	...	...	...	0·7
Feb. 16. 3 0	1·7	−1·4	...	...	...	...	...	0·3	−1·2	...	...	−3·7	...	...	...	...	−0·2	−5·2
Mar. 6. 19 0	−0·8	0·0	...	...	0·3	...	0·5	2·9	...	...	...	...	...	...	...	...	1·5	...
Jan. 6. 7 30	...	0·3	...	...	...	...	...	...	...	...	...	3·3	...	...	...	...	...	2·7
Feb. 1. 3 30	...	−0·8	...	...	...	...	...	1·4	...	...	...	1·0	...	...	...	...	2·7	3·3
Mar. 1. 11 0	...	...	...	...	...	...	...	3·8	...	...	...	...	...	...	...	...	2·8	...
Feb. 7. 19 0	0·3	0·2	...	...	...	...	...	5·3	...	...	...	6·3	...	...	...	...	4·5	4·1
22. 18 45	...	0·3	−0·2	0·2	−0·1	0·0	0·0	2·9	...	...	...	4·5	...	...	...	...	3·3	3·6
28. 12 0	...	0·5	0·4	0·2	0·3	...	0·2	...	...	...	...	...	...	...	...	...	...	...
Mar. 20. 20 10	...	...	...	...	...	...	...	0·8	...	...	...	−0·5	...	...	...	...	1·1	−0·5
Apr. 11. 13 0	...	−0·2	...	...	−0·2	...	−0·2	3·3	...	...	...	...	...	...	...	...	3·8	...
15. 9 0	...	0·2	−0·4	...	−1·0	...	−1·0	4·2	...	...	...	...	...	...	...	...	4·2	6·2
23. 8 55	...	0·1	...	...	0·0	...	−0·2	5·8	...	...	...	...	...	...	...	...	...	...
Jan. 18. 19 0	1·0	0·7	...	...	...	...	...	...	...	...	...	5·1	...	...	...	...	...	...
Feb. 1. 9 0	0·7	0·2	...	...	...	...	...	3·7	4·7	...	...	4·2	...	...	...	...	4·7	4·2
20. 9 10	1·1	1·0	...	...	...	...	...	6·4	...	...	...	7·3	...	...	...	...	...	...
22. 14 30	...	1·4	0·6	0·9	0·9	0·8	1·0	3·6	...	...	...	6·3	...	...	...	...	5·8	4·9
22. 16 0	...	1·3	0·3	0·8	0·6	0·8	0·7	3·1	...	...	...	5·3	...	...	...	...	4·7	4·0
22. 17 30	...	0·8	0·1	0·6	0·2	−0·1	0·1	2·4	...	...	...	4·3	...	...	...	...	3·1	3·2
28. 18 30	...	0·7	0·6	0·8	0·5	...	0·3	...	...	...	...	...	...	...	...	...	4·8	4·0
Apr. 1. 15 30	...	0·7	0·1	...	−1·8	...	−3·4	5·1	...	2·1	...	1·1	...	...	...	...	4·6	2·1
1. 17 0	...	...	...	...	...	...	...	6·5	...	4·5	...	1·5	...	...	...	...	6·5	3·5
17. 17 0	...	0·7	0·3	...	−0·8	...	−0·8	5·7	...	2·2	...	6·4	...	3·2	...	...	6·7	5·7
25. 6 15	...	1·2	1·1	...	0·2	...	0·4	7·2	...	4·4	...	6·4	...	2·4	...	...	...	0·2
Jan. 6. 5 0	0·5	0·1	...	...	...	...	...	...	...	...	...	4·5	...	...	...	...	...	3·3
10. 7 0	0·0	0·2	...	...	...	...	...	...	...	...	...	5·0	...	...	...	...	...	3·3
22. 7 30	2·2	1·8	...	...	...	...	...	...	...	...	...	5·5	...	...	...	...	...	...
Feb. 22. 6 45	...	1·6	1·4	1·4	1·4	1·3	1·3	...	...	...	...	6·4	...	...	...	...	6·6	5·5
22. 12 30	...	2·5	0·3	1·1	0·8	0·8	0·8	3·2	...	...	...	5·3	...	...	...	...	4·5	5·2
23. 13 0	...	2·1	1·1	1·7	1·3	1·3	1·6	3·6	...	...	...	6·3	...	...	...	...	5·8	4·9
28. 17 0	...	1·5	0·9	0·9	0·8	...	0·7	6·2	...	3·7	...	...	...	...	...	...	5·7	4·9
Mar. 12. 9 0	...	1·8	−0·2	...	...	...	...	5·5	...	...	...	...	...	...	...	...	5·8	...
Apr. 1. 14 30	...	1·6	0·4	...	−0·3	...	−2·0	5·7	...	4·7	...	3·1	...	...	...	...	5·7	2·4
5. 17 30	...	0·0	−0·3	...	−0·2	...	−0·4	4·2	...	...	...	...	...	...	...	...	2·8	...
17. 16 0	...	0·7	0·3	...	−1·3	...	−1·8	6·4	...	2·7	...	7·2	3·7	...	...	...	6·7	5·7
29. 9 0	...	1·0	1·0	...	1·0	...	1·0	7·1	...	3·2	...	8·4	8·0	...	...	...	7·3	7·0
Jan. 17. 5 0	...	1·0	...	...	...	...	...	...	...	...	...	5·3	...	...	...	...	6·7	4·6
30. 4 30	...	1·2	...	...	...	...	...	...	...	...	...	5·0	...	...	...	...	6·4	5·2
30. 5 30	...	0·6	...	...	...	...	...	...	...	...	...	4·9	...	...	...	...	5·6	3·5
Feb. 1. 7 0	...	0·8	...	...	...	...	...	3·8	4·0	...	...	5·2	...	...	...	...	6·5	4·8
16. 8 0	...	0·0	...	...	...	...	...	5·5	2·5	...	...	5·7	...	...	...	...	5·8	4·4
Mar. 6. 11 0	...	1·6	...	...	...	...	...	8·8	...	...	...	...	...	...	...	...	4·4	...
20. 9 0	...	1·0	1·0	0·6	0·5	...	0·5	6·0	...	4·7	...	4·5	...	...	...	...	6·5	4·5
20. 10 0	...	1·2	0·8	0·9	0·2	...	0·2	6·2	...	5·2	...	4·2	...	...	...	...	6·4	4·2
20. 11 0	...	1·2	0·8	0·8	0·5	...	0·2	7·0	...	4·0	...	3·8	...	...	...	...	6·2	4·0
20. 11 30	...	1·0	0·7	0·7	−0·4	...	0·6	5·9	...	5·7	...	3·4	...	...	...	...	6·8	4·1
20. 18 20	...	1·7	1·1	1·5	0·5	...	0·5	5·9	...	4·5	...	5·5	...	...	...	...	6·2	4·7
Apr. 1. 18 30	...	2·0	1·5	...	0·0	...	1·3	5·5	...	4·5	...	2·0	...	...	...	...	7·8	2·3
2. 9 0	...	−0·7	−0·7	...	−1·0	...	−0·7	...	...	...	...	...	...	...	...	...	...	...
8. 13 30	...	−1·3	−1·3	...	−1·3	...	...	7·8	...	...	...	3·1	...	...	...	...	7·3	5·1
8. 15 30	...	−0·4	−0·5	...	−0·7	...	...	7·4	...	...	...	3·7	...	...	...	...	7·3	4·9
8. 17 30	...	0·0	−0·1	...	−0·3	...	...	8·1	...	...	...	3·3	...	...	...	...	7·3	4·6
17. 15 0	...	0·0	0·0	...	−1·3	...	−1·3	7·5	...	3·3	...	8·0	3·0	...	...	...	7·5	6·3
17. 18 30	...	−2·0	...	...	−0·5	...	−0·5	...	...	...	...	...	...	...	...	...	3·3	...
23. 7 25	...	0·2	−0·1	...	0·0	...	−0·3	3·8	...	...	...	...	...	...	...	...	...	...
27. 7 3	...	1·1	1·0	...	0·9	...	1·0	3·5	...	...	...	...	...	...	...	...	...	...

1844. Day, hour and minute.	On tin: White on grass.	White one inch high.	Blackened on grass.	On glass on grass.	On glass one inch high.	On pantile.	On slate.	On hare-skin.	On rabbit-skin.	On stone: Firestone.	Purbeck.	Portland.	On Lamb's wool on grass: Yellow.	White.	Jet black.	Crimson.	Orange.	Light blue.	Dark blue.	Green.	Scarlet.	Clouds: Modification.	Amount 0–10.	High or low.	Wind: Direction.	Strength 0–6.	Haze, fog, mist, or vapour.	1844. Day, hour and minute.	Number of the Tables.
h m	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°							h m	
Feb. 21. 11 0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Cirrostratus	10	L.	E.	L.		Feb. 21. 11 0	XXIX.
Apr. 25. 5 45	...	...	...	...	...	...	...	3·2	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			Apr. 25. 5 45	XXX.
Feb. 16. 3 0	0·8	0·3	1·3	2·6	2·1	−1·7	...	...	...	−0·2	−0·2	−0·2	...	...	...	...	...	...	...	...	...	Cumuli......	5					Feb. 16. 3 0	XXXI.
Mar. 6. 19 0	...	...	...	2·5	2·9	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Cirrostratus	7					Mar. 6. 19 0	
Jan. 6. 7 30	4·0	...	4·5	4·3	3·5	2·9	...	...	...	2·7	2·4	2·7	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	½		Jan. 6. 7 30	XXXII.
Feb. 1. 3 30	4·0	2·7	2·4	3·2	0·2	2·7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	1		Feb. 1. 3 30	
Mar. 1. 11 0	...	...	...	3·3	1·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	S.	1		Mar. 1. 11 0	
Feb. 7. 19 0	...	...	...	5·1	4·1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			Feb. 7. 19 0	XXXIII.
22. 18 45	4·7	4·2	4·9	5·3	4·2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	9	H.	S.	¼		22. 18 45	
28. 12 0	5·4	...	4·7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Cirrostratus	1	...	C.	...	Haze	28. 12 0	
Mar. 20. 20 10	...	...	...	3·5	3·5	...	...	1·3	1·0	...	...	...	...	...	...	...	...	...	...	...	...	Thin cirrost.	10	H.				Mar. 20. 20 10	
Apr. 11. 13 0	...	...	...	3·8	3·8																							Apr. 11. 13 0	
15. 9 0	...	...	...	4·2	3·2																							15. 9 0	
23. 8 55	...	...	...	5·7	7·3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			23. 8 55	
Jan. 18. 19 0	5·5	...	5·3	5·7	4·3	...	...	...	...	3·9	4·1	...	...	...	...	...	...	...	...	...	...	Fleecy cl....	5	H.	N.	L.	Haze	Jan. 18. 19 0	XXXIV.
Feb. 1. 9 0	6·2	4·7	5·2	5·4	3·7	4·0	1·2	...	...	2·7	2·0	2·7	...	...	...	...	...	...	...	...	...	Fleecy cl....	3	H.	N.	L.		Feb. 1. 9 0	
20. 9 10	5·0	4·4	5·4	7·5	6·0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	....	...	...	...	...	Fog.	20. 9 10	
22. 14 30	5·8	5·1	6·0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			22. 14 30	
22. 16 0	3·9	3·8	4·6	6·8	5·3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			22. 16 0	
22. 17 30	...	...	...	5·3	4·3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			22. 17 30	
28. 18 30	6·0	...	5·8	2·5	0·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			28. 18 30	
Apr. 1. 15 30	2·3	4·3	5·8	5·1	3·9	...	...	6·9	6·9	...	...	...	5·1	4·1	4·6	3·9	6·0	4·1	5·1	5·3	3·9	Clear	...	...	C.			Apr. 1. 15 30	
1. 17 0	5·6	6·5	8·0	5·2	5·3	...	...	7·3	6·5	...	..	...	6·3	6·0	5·3	6·0	7·0	6·3	7·0	5·5	5·3	Clear	...	...	C.			1. 17 0	
17. 17 0	...	...	...	6·2	5·0	...	...	7·0	6·2	...	...	...	3·7	3·2	3·2	4·4	4·4	3·2	3·7	4·2	3·7	Clear	...	...	C.			17. 17 0	
25. 6 15	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			25. 6 15	
Jan. 6. 5 0	5·5	5·5	...	5·8	4·5	3·1	...	...	...	...	2·7	...	...	...	...	...	...	...	...	...	...	Scud.........	3					Jan. 6. 5 0	XXXV.
10. 7 0	4·7	5·0	...	5·0	4·4	3·7	...	...	...	...	4·0	...	...	...	...	...	...	...	...	...	...	Nearly cl....	3					10. 7 0	
22. 7 30	7·2	...	7·0	9·5	6·8	...	...	...	...	3·5	...	...	...	...	...	...	...	...	...	...	...	Nearly cl....	2	...	C.			22. 7 30	
Feb. 22. 6 45	6·5	5·1	7·1	7·9	6·6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	L.		Feb. 22. 6 45	
22. 12 30	5·5	4·7	5·8	6·3	4·7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	L.		22. 12 30	
23. 13 0	5·8	5·1	6·0	7·5	6·0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			23. 13 0	
28. 17 0	...	...	...	4·2	2·7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	L.		28. 17 0	
Mar. 12. 9 0	...	...	...	6·0	5·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	2	Vap.	Mar. 12. 9 0	
Apr. 1. 14 30	3·4	6·2	7·2	5·7	4·7	...	...	7·7	7·2	...	...	...	7·4	5·7	5·2	6·0	6·7	6·0	6·7	5·1	6·1	Clear	...	...	C.			Apr. 1. 14 30	
5. 17 30	...	...	...	...	3·5	...	...	6·9	6·7	...	...	...	4·2	4·2	4·2	4·7	4·2	4·7	4·4	4·5	4·3	Clear	...	...	C.			5. 17 30	
17. 16 0	...	...	...	6·7	5·7	...	...	11·1	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			17. 16 0	
29. 9 0	...	...	...	5·2	5·0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	A few light c.	½	...	E.	L.		29. 9 0	
Jan. 17. 5 0	6·4	3·2	7·0	6·8	5·6	4·0	3·9	...	...	4·3	3·4	3·5	...	...	...	...	...	...	...	...	...	Light clouds	5	H.	N.	L.	Haze	Jan. 17. 5 0	XXXVI.
30. 4 30	5·4	5·2	5·7	7·9	5·1	...	...	...	...	5·3	4·0	4·6	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	¾		30. 4 30	
30. 5 30	5·6	5·1	6·3	9·4	3·6	...	...	...	...	3·5	2·6	3·1	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	½		30. 5 30	
Feb. 1. 7 0	8·2	5·2	7·5	5·5	5·0	3·5	...	...	...	0·5	2·0	4·2	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	L.		Feb. 1. 7 0	
16. 8 0	4·5	4·5	6·0	7·0	5·0	3·8	...	...	...	3·5	4·0	3·3	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	L.		16. 8 0	
Mar. 6. 11 0	...	...	...	5·8	5·4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	1		Mar. 6. 11 0	
20. 9 0	7·3	...	7·5	7·7	7·7	...	...	12·2	11·5	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	¾		20. 9 0	
20. 10 0	7·2	...	6·7	7·2	6·2	...	...	11·2	12·7	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	½		20. 10 0	
20. 11 0	6·5	...	6·0	7·0	5·5	...	...	11·0	13·0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	½		20. 11 0	
20. 11 30	6·7	...	7·0	7·7	6·4	...	...	10·9	11·4	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	¼		20. 11 30	
20. 18 20	4·5	...	5·0	8·5	5·9	...	...	7·5	6·1	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			20. 18 20	
Apr. 1. 18 30	...	...	...	7·0	7·0	...	...	6·5	6·5	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			Apr. 1. 18 30	
2. 9 0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	....	...	...	S.	1		2. 9 0	
8. 13 30	...	...	...	9·9	6·4	...	...	7·1	10·8	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			8. 13 30	
8. 15 30	...	...	...	10·1	7·7	...	...	8·9	9·5	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			8. 15 30	
8. 17 30	...	...	...	10·0	8·1	...	...	10·4	10·8	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			8. 17 30	
17. 15 0	...	...	...	7·7	6·5	...	...	7·5	7·0	...	...	...	5·0	4·7	4·7	4·5	4·8	6·5	5·3	5·0	5·2	Clear	...	...	C.			17. 15 0	
17. 18 30	...	...	...	5·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			17. 18 30	
23. 7 25	...	...	...	5·0	7·0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			23. 7 25	
27. 7 3	...	...	...	9·5	9·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	C.			27. 7 3	

Feb. 21. 11h 0m: a thin rain was falling. Feb. 16. 3h 0m: the sun is shining brightly. Feb. 22. 18h 45m, the sun is high and ...

Table (Continued).

Excess of the reading of the thermometer in air, above that placed

Number of the Tables.	1844. Day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	On long grass.	On short grass.	On white raw wool.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.	On long grass covered by White raw wool.	Flax.	White tin.	White tin one inch high.	Blackened tin.	Lead.	Lead six inches high.	Glass.	Glass one inch high.	Hare skin.	Rabbit skin.
XXXVII.	Jan. 22. 10 30	34·8	9·2	8·6	13·5	8·6	7·6	2·8	0·2	3·0	1·0	...	...	...	...	...	...	...	6·2	4·3	...	...
	26. 7 30	42·5	9·2	6·9	6 9	5·5	7·3	2·9	4·3	7·3	5·4	...	...	...	...	6·0	...	...	7·0	4·5	...	...
	Feb. 1. 5 0	33·5	9·0	8·0	10·3	7·3	10·0	6·0	−2·0	0·0	−0·5	3·0	1·0	3·5	3·0	7·0	...	4·5	2·0	4·5	...	...
	15. 19 15	34·0	9·8	8·0	7·0	8·5	8·5	4·0	−1·2	1·0	−1·0	2·0	4·0	3·5	6·0	2·0	3·5	2·0	5·0	4·5	...	...
	16. 7 0	39·2	9·0	7·2	6·7	7·7	8·5	2·7	1·7	3·0	2·7	0·7	1·7	2·2	2·2	5·4	3·7	3·7	7·2	4·2	...	...
	16. 9 0	36·8	9·3	6 8	5·8	7·8	7·6	2·8	0·7	1·3	0·8	1·2	1·1	2·3	1·3	4·3	4·0	3·6	5·6	3·6	...	...
	20. 5 0	36·7	9·3	7·2	10·5	9 1	8·7	3·4	0 1	2·3	1·5	0·7	−0·1	−0·4	0·0	...	...	...	...	...	...	...
	Apr. 1. 12 30	42·7	9·5	8·2	14·5	12·7	6·7	2·7	−2·8	3·7	−2·3	−2·3	−2 3	...	...	...	...	...	6·7	5·7	−1·3	−0·8
	1. 13 30	41·3	9·4	9·3	13·3	13·3	6·3	3·1	−3·7	1·3	−2·8	−4·2	−4 2	...	...	...	...	...	6·3	4·3	−3·7	−3 7
	1. 18 0	39·7	9·2	8·9	11·2	9 9	6·7	5·2	−3·3	2·7	1·3	−4·3	−4·3	...	...	...	...	...	5·7	3·7	−3·8	−3·8
	2. 7 0	51·9	9·9	6·1	9·9	9·9	8·9	2·9	−0·6	1·9	−0·1	...	−1·6	...	...	...	...	...	6 9	4·9	...	...
	3. 17 30	45·6	9·0	7·9	12·6	15·9	7·0	3·0	1·0	5·1	2·2	...	...	...	...	...	...	...	6·4	5·5	...	...
	17. 12 0	48·5	9·3	9·6	14·5	15·3	5·5	2·5	0·5	7·0	3·5	−2·5	−1·5	...	...	...	...	...	7·5	7·5	0 5	0·5
	29. 11 0	39·4	9·6	8·3	12·1	12·8	7·5	4·9	−6·9	−0·4	2·3	...	...	...	...	...	...	...	...	...	...	...
	30. 7 40	48·6	9·0	7·3	8·4	7·8	6·6	2·4	−5·8	−0·3	−2·6	...	...	...	...	...	...	...	...	...	...	...
	Feb. 13. 20 0																					
XXXVIII.	Jan. 26. 5 10	42 8	10·3	7·8	9·8	6·8	8·0	3·3	2·8	6·3	5·1	...	...	...	...	5·7	...	...	6·7	4·8	...	...
	Feb. 15. 15 0	36·8	10·6	8·3	10·8	10·3	9·4	4·4	−0·2	3·3	0·3	2·3	2·8	6·3	5·3	6·3	4·8	5·3	6·3	3·8	...	...
	15. 17 30	34·0	10·8	7·8	7·0	10·5	10·0	4·5	−2·0	1·0	−1·0	2·0	2·0	6·0	6·5	4·2	4·7	2·0	6 5	3·5	...	...
	16. 4 30	42·7	10·2	8·9	7·5	9·2	9·5	3·2	3·2	4·9	4·5	4·2	3 9	3·7	3·7	4·7	5·7	4·7	5·7	5·7	...	...
	22. 9 0	28·1	10·1	7·3	11·6	10·1	11·9	−1·6	−5·9	−3·3	−4·9	...	...	...	...	...	...	...	...	...	...	...
	Mar. 21. 8 40	35·7	10·2	10·2	15·5	15·5	7·7	2·7	...	...	0·2	...	...	...	...	...	...	...	5·0	7·1	...	...
	Apr. 2. 8 0	49·2	10·0	9·0	11·9	10·2	6·7	4·2	−0·8	3·2	1·9	0·8	−0·8	...	...	...	...	...	4·2	2·0	1·2	1·2
	2. 11 0	47·0	10·0	9·2	12·3	10·0	...	6·0	−0·6	1 0	2·0	7·0	4·0	...	...	...	...	...	5·5	3·7	6·0	5·7
	8. 11 0	44·4	10·4	10·0	14·2	14·2	6·9	1·8	1·0	4·3	1·4	...	...	...	...	...	...	...	...	...	...	...
	9. 8 50	51·6	10·2	8·6	12·4	11·6	5·4	2·3	2·3	6·6	3·0	...	...	...	...	...	...	...	9·5	4·6	...	...
	17. 13 0	48·8	10·8	10·8	14·8	15·8	6 8	2·8	0·8	6·3	3·3	−1·2	−1·2	...	...	...	...	...	8·3	8·3	0·8	0·8
	17. 14 0	47·2	10·2	9·2	14·7	15·2	6·2	3·7	0·2	4·7	2·2	−3 3	−3·3	...	...	...	...	...	7·4	6·2	0·2	0·2
	17. 18 0	44·2	10·2	9·7	14·7	15·7	5·2	2·2	−1·3	5·0	−0·8	−0·8	−0·8	...	...	...	...	...	5·2	5·2	2·2	2·2
	18. 17 30	36·3	10·1	9·1	12·5	13 5	11·0	4·7	...	...	−3·0	...	...	...	...	...	...	...	...	...	...	...
	25. 6 40	57·7	10·5	8·9	9·2	8·7	6·7	2·7	0·2	2 2	1·2	...	...	...	...	...	...	...	...	...	...	...
	25. 17 20	41·6	10·8	9·3	12·1	11·1	5·3	3·6	−3·4	4·6	1·9	...	...	...	...	...	...	...	...	...	...	...
	27. 9 30	45·6	10·6	9·6	13·0	14·3	11·6	4·0	−6·4	−0·2	−1·2	...	...	...	...	...	...	...	...	...	...	...
	May 1. 9 5	49·7	10·7	10·7	12·5	13·9	10·8	3·2	−1·6	5·4	0 9	...	...	...	...	...	...	...	...	...	...	...
	1. 11 0	46·7	10·7	7·9	10·7	10·2	7·2	3·2	−1·3	4·8	0·5	1·7	1·7	...	...	...	...	...	6·2	6·2	2·7	...
XXXIX.	Jan. 22. 8 15	36·1	11·7	10·3	16·3	10·7	10·6	3·3	0·2	1·2	3·2	...	...	...	...	...	...	...	6·0	6·0	...	...
	22. 9 0	35·7	11·7	9·9	16·1	10·8	10·7	3·1	0·3	1·1	3·9	...	...	...	...	...	...	...	5·4	5·5	...	...
	Feb. 15. 18 30	33·5	11·5	9·0	10 0	10·5	7·5	3·5	2·0	0·7	1·7	1·5	2·5	3·0	3·5	4·5	4 5	2·0	2·0	3·4	...	...
	22. 11 30	26·7	11·5	10·2	13·3	13·7	16·1	5·7	−4·8	−3·2	−3·4	...	...	...	...	...	...	...	6·8	4·4	...	...
	Mar. 21. 6 30	39·4	11·4	11·2	13·4	13·2	9·4	3·1	...	5·4	3·1	7·4	5·2	...	...	...	...	...	6·4	5·2	...	...
	23. 8 40	38·5	11·3	9·0	10·7	12·0	8·7	2·9	...	1·4	0·0	−0·3	0·5	...	...	...	...	...	7·2	6 3	...	...
	Apr. 1. 9 0	48·5	11·3	9·9	11·3	12·5	8·3	2·3	...	...	1·2	...	...	...	...	...	...	...	6·5	6·5	...	...
	3. 9 0	47·5	11·7	10·3	17·5	11·7	9·5	2·3	−0·8	...	0·5	...	...	...	...	...	...	...	6·5	4·8	...	...
	3. 11 30	45 9	11·4	8·4	12·4	13·2	9·4	2·1	−1·2	4·7	3·7	...	...	...	...	...	...	...	6·4	5·7	...	...
	3. 12 45	45·9	11·9	9·7	13·9	15·4	10·3	2·3	−0 3	4·9	2·9	...	...	...	...	...	...	...	6·4	5·9	...	...
	3. 15 30	45·8	11·3	9·0	14·5	17·0	9·8	2·8	0·3	5·0	2·7	...	...	...	...	...	...	...	6·3	5·5	...	...
	10. 7 0	55·7	11·9	10·7	13·7	12·7	6·3	2·2	...	...	...	...	...	...	...	...	...	...	5·5	4·9	...	...
	17. 9 30	52·5	11·2	9·9	15 4	16·5	8·8	3·5	...	...	...	...	...	...	...	...	...	...	8·3	6·1	...	...
	May 1. 7 30	53·1	11·1	10 5	12·1	10·1	10·6	3·1	−0·2	4·1	0·1	...	...	...	...	...	...	...	...	...	...	...
	1. 15 0	44·8	11·3	8·8	11·8	12·3	7·3	2·6	−2·7	2·3	−0·7	−0·2	−3·2	...	...	...	...	...	6·3	5·3	...	...
XL.	Feb. 16. 5 30	42·0	12·0	10·0	11 5	12·0	10·0	4·5	3·0	4·5	4·8	4·5	4 5	5 0	8·0	5·0	8·5	6·5	9·2	6·5	...	...
	Apr. 1. 12 0	44·4	12·9	12·2	16·4	16·4	9·4	4·6	−1·6	−4·6	−1·8	−3·6	...	...	...	...	...	...	5·9	6·6	−1·3	−1·1
	10. 8 0	53·5	12·5	...	11·5	10·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	10. 9 0	50·8	12·6	11·5	11·6	15·2	7·1	0·0	...	...	...	...	...	...	...	...	...	...	9·6	6·3	...	...
	18. 9 0	47·5	12·0	9·5	14·5	14·5	8·0	2·3	1·5	6·5	2·3	1·5	...	...	...	...	...	...	9·7	7·7	...	...
	25. 11 30	47·9	12·4	9·7	13·4	12·9	8·9	3·9	−3·3	4·7	0·1	...	...	...	...	...	...	...	...	...	...	...
	25. 16 30	41·7	12·4	10·4	15·7	15·5	9·7	6·3	−3·5	6·6	1·8	...	...	...	...	...	...	...	...	...	...	...
	May 1. 15 0	41·2	12·2	10·2	15·2	15·2	6·7	5·7	−1·8	4 2	−1·2	−3·8	...	...	...	...	...	...	7·2	6·4	...	...
	1. 17 0	40·7	12·2	12·2	12·7	16 5	10·7	5·2	−2·3	3·7	−0·7	−2·8	...	...	...	...	...	...	...	...	...	...
	13. 19 0	54 0	12·7	11·0	15·7	14·0	8·0	4·0	−8·0	...	...	...	...	...	...	...	...	...	6·2	6 0	...	...

1844. Day, hour and minute.	Above grass in feet. 1.	2.	4.	6.	8.	10.	12.	On lead. On grass.	Six inches high.	One foot high.	Three feet high.	On zinc. On grass.	Six inches high.	One foot high.	Three feet high.	Four feet high.	On copper on grass.	On iron on grass.	On tin. White on grass.	White one inch high.	Blackened on grass.	On glass on grass.	On glass one inch high.	On pantile.	On slate.	On hare-skin.	On rabbit-skin.
Jan. 22. 10 30	0·4	0·8	...	...	...	...	...	...	...	...	...	...	...	...	...	...	8·3	6·4	8·6	...	9·0	8·5	5·6	...	...	...	...
26. 7 30	0 9	0·7	...	...	...	...	...	...	...	...	...	5·3	...	...	...	...	7·5	5·0	5·3	3·9	6·5	7·7	5·7	...	...	...	...
Feb. 1. 5 0	2·5	2·0	...	...	...	...	...	8·3	4·5	...	...	6·3	...	...	...	...	9·5	7·0	7·0	7·0	7·0	7·5	7·0	3·0	...	...	...
15. 19 15	2 0	2·0	...	...	...	...	...	6·5	5·0	...	...	8·0	...	...	...	...	7·2	5·0	4·5	4·5	6·0	7·0	5·0	3·8	...	...	...
16. 7 0	0·7	0·7	...	...	...	...	...	5·7	3·2	...	...	6·0	...	...	...	...	6·3	5·6	6·5	6·1	6·2	7·7	6·2	6·2	...	...	...
16. 9 0	0·8	0·6	...	...	...	...	...	6·3	2·8	...	...	5·6	...	...	...	...	...	6·0	5·8	4·8	5·8	7·6	4·8	4·7	...	...	...
20. 5 0	2·3	1·7	...	...	−0·1	...	...	6·9	...	...	...	6 9	...	...	...	...	9·2	4·5	6·9	6·5	7·4	8·7	8·0	4·7	...	...	...
Apr. 1. 12 30	...	0·9	0·5	...	−0·3	...	−0·8	9·2	...	5·7	...	2·1	...	...	...	...	6·3	3·3	4·9	7·9	7·9	8·7	7·9	...	...	9·2	9·2
1. 13 30	...	2·5	0·8	...	1·9	...	−2·0	8·3	...	5 3	...	1·5	...	...	...	...	6·7	4·2	6·3	8·3	9·3	7·1	6·3	...	...	7·8	8·1
1. 18 0	...	3·0	2·5	...	...	...	1·7	7·7	...	4·5	...	2·7	...	...	...	...	4·9	2·9	6·7	8·7	9·2	5·6	6·5	...	...	8·7	9·2
2. 7 0	...	−1·6	−2·1	...	−4·1	...	−1·9	6·4	...	2·7	...	2·4	...	...	...	...	6·6	3·1	5·4	4·7	4·9	6·9	4·1	...	...	6·9	6·9
3. 17 30	...	−0·8	−0·9	...	−0·9	...	...	9·0	...	...	...	...	...	...	...	...	9·3	5·5	...	...	...	7·8	6·1	...	...	10·6	10·4
17. 12 0	...	0·5	0·0	...	−1·3	...	−1·3	8·7	...	5·5	...	4·0	...	...	...	...	9·2	8·3	...	...	...	8·5	7·5	...	...	10·3	9·5
29. 11 0	...	1·6	1·2	...	1·2	...	...	8·4	...	4·7	...	8·5	5·2	...	...	...	6·2	5·3	...	...	...	...	...	...	...	...	...
30. 7 40	...	1·4	0·8	...	1·2	...	1·1	5·5	...	3·7	...	6·4	4·0	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Feb. 13. 20 0	...																										
Jan. 26. 5 10	1·0	0·7	...	...	...	...	...	...	...	...	...	5·7	...	...	...	...	9·0	6·8	6·5	5·3	7·1	9·2	7·0	...	...	...	...
Feb. 15. 15 0	2·3	2·1	...	...	...	...	...	6·3	5·0	...	...	6·3	...	...	...	...	6·0	6·8	9·0	6·8	8·8	7·3	4·8	4·8	...	...	...
15. 17 30	2·0	2·0	...	...	...	...	...	7·5	4·9	...	...	6·7	...	...	...	...	9 0	6·5	10·0	7·8	8·5	9·0	6·5	3·5	...	...	...
16. 4 30	1·7	1·2	...	...	...	...	...	7·7	4·5	...	...	8·5	...	...	...	...	8·9	6·7	7·7	9·9	7·7	10·5	7·7	6·5	...	...	...
22. 9 0	...	2·1	1·1	1·1	0 8	0·8	0·1	8·0	...	...	...	7·4	...	...	...	...	7·3	6·6	8·1	...	8·1	9·5	7·7	...	...	...	...
Mar. 21. 8 40	...	−0·9	−0 8	1·1	−0·5	...	...	6·4	...	...	...	...	...	...	...	...	6·5	...	...	...	...	8·7	5·9	...	...	...	...
Apr. 2. 8 0	...	−1·3	−1·3	...	−1·3	...	−1·3	7·2	...	1·0	...	2·2	...	...	...	...	6·2	3·0	4·4	3·2	4·4	6·2	3·2	...	5·7	8·2	...
2. 11 0	...	−1·4	−1·4	...	−1·0	...	−2·6	...	...	...	...	...	...	...	...	...	6·0	...	...	...	...	5·2	4·5	...	7·5	8·8	...
8. 11 0	...	−0·7	−0·7	...	−0·6	...	...	7·9	...	...	...	2·7	...	...	...	...	7·9	5·4	...	...	...	10·3	7·5	...	7·4	8·8	...
9. 8 50	...	−0·6	−0·6	...	−0·9	...	...	6·8	...	...	...	...	...	...	...	...	6·5	3·6	...	...	...	10·4	7·4	...	7·4	7·2	...
17. 13 0	...	0·6	0 6	...	−0·2	...	−0·2	8·8	...	5·8	...	7·8	5·0	...	...	...	8 8	6·3	...	...	...	8·8	8·8	...	10·8	9 8	...
17. 14 0	...	1·0	0·7	...	−0·8	...	−0·8	8·7	...	5·2	...	10·2	6·2	...	...	...	7·7	9·7	...	...	...	8·2	7·2	...	10·7	10·0	...
17. 18 0	...	0·0	0·0	...	0·0	...	0·0	8·0	...	4·7	...	8·0	4·2	...	...	...	7·2	6·4	...	...	...	6·2	5·2	...	7·2	6·7	...
18. 17 30	...	1·6	1·3	...	1·5	...	1·7	10·3	...	...	...	8·7	...	...	...	...	6·7	...	...	...	...	8·5	8·3	...	13·3	10·1	...
25. 6 40	...	2·6	2·4	...	2·3	...	2·5	8·2	...	5·2	...	9·6	4·4	...	...	...	7·2	...	...	...	...	...	...	...	10·9	...	...
25. 17 20	...	−0·2	−0·2	...	0·0	...	−0·1	10·5	...	6·6	...	...	...	...	...	...	9·6	8·3	...	...	...	...	...	...	10·1	...	...
27. 9 30	...	2·6	2·0	...	1·6	...	1·6	7·6	...	...	...	...	...	...	...	...	...	...	...	...	...	10·8	9·4	...	...	...	...
May 1. 9 5	...	2·3	2·0	...	1·0	...	1·2	8·9	...	...	...	...	...	...	...	...	...	...	...	...	...	10·7	9·3	...	...	...	...
1. 11 0	...	1·7	1·3	...	1·2	...	1·2	8·2	...	4·2	...	7·2	4·5	...	3·5	2·9	8·7	6·2	...	...	...	8·9	7·7	...	9·5	9·2	...
Jan. 22. 8 15	1·5	1·3	...	...	...	...	...	...	...	...	...	4·6	...	...	...	...	...	6·6	11·7	...	11·4	11·1	9·4	...	...	...	...
22. 9 0	1·2	1·0	...	...	...	...	...	...	...	...	...	4·6	...	...	...	...	...	6·7	11·2	...	10·9	10·7	8·7	...	...	...	...
Feb. 15. 18 30	1·8	1·5	...	...	...	...	...	7·3	6·3	...	...	5·5	...	...	...	...	9·5	5·3	10·0	10·5	6·5	9·5	6·0	3·0	...	...	...
22. 11 30	...	2·7	2·4	2·5	2·4	2·1	2·4	5·7	...	...	...	7·7	...	...	...	...	7·4	6·4	10·0	7·3	10·5	11·2	9·4	...	...	...	...
Mar. 21. 6 30	...	0·2	0·1	...	...	...	...	7·4	...	...	...	...	...	...	...	...	9·3	...	...	...	...	10·3	8·4	...	...	11·4	8·9
23. 8 40	...	0·3	0·0	...	−0·8	...	...	7·5	...	...	...	...	...	...	...	...	8·4	5·7	6·0	...	...	10·1	8·3	...	...	10·5	10·0
Apr. 1. 9 0	...	0·1	0·5	...	...	...	...	8·5	...	...	...	...	...	...	...	...	8·5	4·5	...	...	...	8·5	7·5	...	...	11·5	10·0
3. 9 0	...	0·2	0·5	...	−0·7	...	−0·8	9·3	...	...	...	...	...	...	...	...	6·5	...	...	...	...	9·5	7·5	...	...	11·5	8·7
3. 11 30	...	−0·3	−0·4	...	−0·4	...	...	7·6	...	...	...	...	...	...	...	...	7·9	3·8	...	...	...	8·1	6·9	...	...	9·3	11·9
3. 12 45	...	−0·3	−0·3	...	−0·3	...	...	8·3	...	...	...	...	...	...	...	...	7·9	3·9	...	...	...	8·4	7·1	...	...	11·3	11·1
3. 15 30	...	−0·4	−0·4	...	−0·4	...	...	8·7	...	...	...	...	...	...	...	...	7·6	3·8	...	...	...	8·3	6·8	...	...	11 4	10·4
10. 7 0	...	0·0	0·9	...	...	...	...	6·5	...	...	...	...	...	...	...	...	7·5	...	...	...	...	6·7	6·5	...	...	12·6	12·0
17. 9 30	...	1·6	1·6	...	0·7	...	...	8·9	...	...	...	...	...	...	...	...	8·2	5·4	...	...	...	10·5	8·7	...	...	...	...
May 1. 7 30	...	2·3	2·1	...	1·5	...	1·7	8·9	...	...	...	...	...	...	...	...	...	...	...	...	...	10·8	9·8	...	...	...	...
1. 15 0	...	2·0	1·6	...	1·1	...	1·1	7·3	...	4·3	...	7·2	5·6	...	4·5	4·1	8·6	8·6	...	...	...	7·6	5·8	...	...	13.3	11·8
Feb. 16. 5 30	2·0	1·5	...	...	...	...	...	10·0	7·0	...	...	9·0	...	...	...	...	9·5	7·2	9·8	6·8	9·0	10·8	8·5	7·0	...	...	...
Apr. 1. 12 0	...	1·4	1·2	...	0·4	...	0·6	10·9	...	5·6	...	3·9	...	...	...	...	11·4	6·9	5·9	9·2	8·6	11·9	10·4	...	...	13·9	12·9
10. 8 0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
10. 9 0	...	...	...	...	...	...	...	8·6	...	...	...	...	...	...	...	...	8·5	...	...	...	...	12·8	7·6	...	...	8·3	10·8
18. 9 0	...	1·3	1·3	...	−0·5	...	−0·5	7·5	...	5·5	...	12·3	6·5	...	...	...	10·3	8·5	...	...	...	10·3	10·3	...	...	10·5	9·5
25. 11 30	...	1·9	1·6	...	1·6	...	...	11·7	...	8·5	...	11·5	6·6	...	...	...	11·5	...	...	...	...	...	...	...	...	12 0	...
25. 16 30	...	2·1	0·9	...	1·6	...	1·1	12·0	...	7·3	...	10·8	5·9	...	...	...	11·9	9·2	...	...	...	...	...	...	...	12·3	...
May 1. 15 0	...	2·4	1·9	...	1·4	...	1·5	9·2	...	6·7	...	10 4	7·2	...	5·3	4·0	10·4	10·0	...	...	...	9·0	9·0	...	...	12·7	11·2
1. 17 0	...	2·5	2·0	...	1 5	...	1·4	10·2	...	4 2	...	8·5	4·7	...	2·7	2·2	10·2	8·8	...	...	...	10·5	9·7	...	...	13·2	12·7
13. 19 0	...	2·0	...	...	1·0	...	1·0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	12·0	9·0	...	...	...	...

1844. Day, hour and minute.	On stone. Firestone.	Purbeck.	Portland.	On Lamb's wool on grass. Yellow.	White.	Jet black.	Crimson.	Orange.	Light blue.	Dark blue.	Green.	Scarlet.	Clouds. Modification.	Amount 0–10.	High or low.	Wind. Direction.	Strength 0–6.	Haze, fog, mist, or vapour.	1844. Day, hour and minute.	Number of the Tables.
Jan. 22. 10 30	3·5	2·8	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.	...		Jan. 22. 10 30	XXXVII.
26. 7 30	4·7	4·5	4·1	...	...	...	...	...	...	...	...	...	Clear	...	...	N.	½	Haze	26. 7 30	
Feb. 1. 5 0	4·5	4·0	6·0	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	L.		Feb. 1. 5 0	
15. 19 15	3·5	4·0	3·3	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	L.		15. 19 15	
16. 7 0	4·4	5·4	4·7	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	½	Vap.	16. 7 0	
16. 9 0	4·6	5·1	4·0	...	...	...	...	...	...	...	...	...	Clear	...	...	W.	½		16. 9 0	
20. 5 0	4·5	4·7	4·9	...	...	...	...	...	...	...	...	...	A thin cloud	10					20. 5 0	
Apr. 1. 12 30	...	...	...	7·5	7·9	5·5	7·2	8·2	8·2	6·7	7·7	6·6	Clear	...	...	c.			Apr. 1. 12 30	
1. 13 30	...	...	...	8·3	4·3	4·3	4·3	5·3	5·8	7·8	6·8	7·1	Clear	...	...	c.			1. 13 30	
1. 18 0	...	...	...	5·9	5·9	2·5	5·2	6·7	5·9	5·7	4·2	4·5	Clear	...	...	c.			1. 18 0	
2. 7 0	...	...	...	...	...	...	...	...	...	...	...	...	Cirrostratus	1	...	S.	1		2. 7 0	
3. 17 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			3. 17 30	
17. 12 0	...	...	...	6·5	7·5	5·3	6·0	6·0	6·5	7·3	6·0	5·3	Clear	...	...	S.	1		17. 12 0	
29. 11 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	E.	1		29. 11 0	
30. 7 40	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	E.	1		30. 7 40	
Feb. 13. 20 0																			Feb. 13. 20 0	
Jan. 26. 5 10	5·3	5·0	4·8	...	...	...	...	...	...	...	...	...	Nearly cl....	1	...	N.	½	Haze	Jan. 26. 5 10	XXXVIII.
Feb. 15. 15 0	4·3	4·6	4·3	...	...	...	...	...	...	...	...	...	....	2	...	N.	L.	Vap.	Feb. 15. 15 0	
15. 17 30	2·8	5·0	2·8	...	...	...	...	...	...	...	...	...	....	1	...	N.	L.	Vap.	15. 17 30	
16. 4 30	4·5	5·7	4·7	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			16. 4 30	
22. 9 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			22. 9 0	
Mar. 21. 8 40	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.	...	Haze	Mar. 21. 8 40	
Apr. 2. 8 0	...	...	...	...	...	...	...	...	...	...	...	...	....	...	...	S.	¾		Apr. 2. 8 0	
2. 11 0	...	...	...	...	...	...	...	...	...	...	...	...	....	...	...	S.	L.		2. 11 0	
8. 11 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			8. 11 0	
9. 8 50	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			9. 8 50	
17. 13 0	...	...	...	6·3	7·8	5·8	8·0	7·6	8·8	7·7	7·8	7·6	Clear	...	...	c.			17. 13 0	
17. 14 0	...	...	...	6·2	6·0	6·0	6·7	6·0	7·0	6·0	6·0	5·2	Clear	...	...	c.			17. 14 0	
17. 18 0	...	...	...	3·7	5·7	5·7	4·4	5·0	5·4	4·0	5·0	4·7	Clear	...	...	c.			17. 18 0	
18. 17 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			18. 17 30	
25. 6 40	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			25. 6 40	
25. 17 20	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			25. 17 20	
27. 9 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			27. 9 30	
May 1. 9 5	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			May 1. 9 5	
1. 11 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			1. 11 0	
Jan. 22. 8 15	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Jan. 22. 8 15	XXXIX.
22. 9 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			22. 9 0	
Feb. 15. 18 30	2·5	4·5	1·5	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Feb. 15. 18 30	
22. 11 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear						22. 11 30	
Mar. 21. 6 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.		Haze	Mar. 21. 6 30	
23. 8 40	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			23. 8 40	
Apr. 1. 9 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Apr. 1. 9 0	
3. 9 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			3. 9 0	
3. 11 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	S.	L.		3. 11 30	
3. 12 45	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	S.	L.		3. 12 45	
3. 15 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	S.	L.		3. 15 30	
10. 7 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			10. 7 0	
17. 9 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			17. 9 30	
May 1. 7 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			May 1. 7 30	
1. 15 0	...	...	...	10 0	8·3	8·1	8·6	10·0	8·3	9·6	8·1	10·0	Clear	...	...	c.			1. 15 0	
Feb. 16. 5 30	5·0	5·8	5·0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Feb. 16. 5 30	XL.
Apr. 1. 12 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Apr. 1. 12 0	
10. 8 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			10. 8 0	
10. 9 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			10. 9 0	
18. 9 0	...	...	...	9·3	9·3	10·3	8·3	8·0	10·0	8·7	8·3	9·0	A thin cloud	...	...	W.	L.		18. 9 0	
25. 11 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			25. 11 30	
25. 16 30	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			25. 16 30	
May 1. 15 0	...	...	...	9·7	9·4	9·2	9·2	9·7	10·7	11·7	7·2	11·7	Clear	...	...	c.			May 1. 15 0	
1. 17 0	...	...	...	11·2	10·2	8·7	10·7	9·7	11·5	9·2	9·5	9·2	Clear	...	...	c.			1. 17 0	
13. 19 0	...	...	...	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			13. 19 0	

Feb. 13, 20h. The minimum temperature of the air the preceding night was 19°, and the lowest reading on raw wool was 7°·8. (See section of self-registering thermometers.)

The appearance of trees and shrubs as covered with hoar-frost was very rich, and on examination it appeared that the edges of the leaves of laurel were fringed with spikes one-tenth of an inch in length, and inclined to the leaf, both upwards and downwards, at an angle of about 60°; none were in the plane of the leaf continued; on the surface of the leaf there were spikes one-sixteenth of an inch in length down each fibre; the intermediate spaces on the leaf itself were covered with small particles. Privet: every leaf was covered from the edge for one-fourth of an inch within the leaf, with spikes one-fourth of an inch in length; the other parts of the leaf were free from frost; the cruciform end of each branch was very rich with spikes. Broom: this shrub was richly encrusted with spikes all around each branch, by far the greater number, and much larger than the rest springing from the sides; from part of each branch towards the zenith there were comparatively but few spikes, and none of them vertical: this shrub, from its graceful form, had a peculiarly rich appearance. Grass: the lower part of the blade was free from frost; immediately above this the blade was just covered with white particles, which increased to spikes, becoming longer and longer as the distance from the root increased, terminating at the top in a rich cluster of spikes one-tenth of an inch in length, and inclined at all angles from 0° to 90° all round; the spikes on the blade below the top were nearly horizontal, and in the plane of the blade. Wooden palings: at all points and angles there were clusters of spikes half an inch in length. Glass one inch high was covered with spikes one-sixteenth of an inch in length, with horizontal spikes one-tenth of an inch in length from its edges. Glass on grass was free of hoar-frost, and so were copper, lead, zinc, tin, and iron. Tin one inch high was a little white at the edges. Wood on grass was free from hoar-frost; wood raised from the ground was covered with white particles, and many spikes in the direction of the fibres. Raw wool was richly encrusted in each fibre, and in some parts spikes were piled on spikes, forming a cluster three-fourths of an inch in length. On flax there were no spikes, it being covered with white round particles on each fibre. Stone was free from hoar-frost.

Table (Continued).

Number of the Tables.	1844. Day, hour and minute.	Reading of Therm. at the height of 4 feet above the soil, protected from radiation.	Excess of the reading of the thermometer in air, above that placed								
	h m		On long grass.	On short grass.	On white raw wool.	On flax on grass.	In focus of reflector.	Nine inches above wood.	One inch below surface of ground under short grass.	On surface of ground under short grass.	On surface of ground under long grass.
XLI.	Mar. 23. 7 45	40·9	13·6	11·1	14·9	15·7	10·1	3·8	...	3·2	2·1
	Apr. 3. 7 0	51·3	13·1	11·5	18·3	17·3	9·2	2·3	0·4	...	3·6
	6. 7 25	49·2	13·2	10·2	14·2	13·2	9·2	3·5	0·2	...	3·0
	9. 7 50	52·3	13·1	10·8	17·1	17·8	9·3	1·7	2·1	7·6	4·5
	17. 11 0	48·5	13·0	12·0	18·5	18·5	7·5	3·0	0·0	6·0	5·0
	18. 11 5	42·5	13·5	12·0	17·5	17·7	12·2	0·7	...	...	0·7
	24. 14 40	40·0	13·2	12·0	18·0	18·0	10·4	6·0	...	...	...
XLII.	Apr. 24. 9 30	48·5	14·5	12·5	18·0	16·5	9·5	5·5	1·0	6·5	3·0
	24. 10 0	47·5	14·5	12·3	18·5	16·3	9·8	5·3	0·5	7·0	1·5
	24. 10 30	46·8	14·8	12·7	18·3	16·8	8·8	6·1	−0·2	6·3	1·6
	24. 12 40	44·1	14·5	12·7	17·9	16·9	8·1	6·6	...	...	...
	24. 13 40	42·8	14·4	10·8	17·3	15·8	9·6	5·8	...	...	...
	25. 15 40	42·3	14·3	12·1	17·2	17·8	12·1	6·2	−4·3	8·3	0·4
XLIII.	Apr. 1. 11 0	46·7	15·2	13·2	17·2	17·2	9·7	5·7	0·7	3·7	1·2
	8. 9 0	46·3	15·9	14·4	21·3	22·3	11·3	...	...	...	...
	9. 7 30	53·8	15·8	14·3	14·8	16·0	13·8	1·2	2·6	8·6	4·0
	24. 9 0	49·5	15·5	13·5	17·5	17·5	10·5	5·3	1·5	7·5	1·5
	24. 11 0	45·7	15·2	12·5	19·2	16·5	10·2	6·8	−1·3	8·0	0·7
	24. 11 30	45·6	15·6	13·1	19·6	18·1	10·2	7·1	−1·4	8·6	0·6
	24. 12 0	44·7	15·2	12·7	19·5	18·2	9·7	7·2	−2·3	7·2	−0·3
XLIV.	Apr. 8. 7 20	52·7	19·2	17·5	21·7	22·7	12·7	...	2·7	...	8·7
	8. 8 0	48·0	19·6	18·0	25·0	23·0	12·0	...	...	...	...

Number of the Tables.	1844. Day, hour and minute.	On long grass covered by										
		White raw wool.	Flax.	White tin.	White tin one inch high.	Blackened tin.	Lead.	Lead six inches high.	Glass.	Glass one inch high.	Hare skin.	Rabbit skin.
XLI.	Mar. 23. 7 45	1·9	3·5	...	...	...	...	...	6·4	7·8	...	...
	Apr. 3. 7 0	...	...	...	...	...	...	...	...	5·3	...	...
	6. 7 25	...	...	...	...	...	...	...	...	...	...	...
	9. 7 50	...	...	...	...	...	...	...	10·5	7·7	...	...
	17. 11 0	−2·5	−3·5	...	...	...	...	...	10·0	6·5	−1·1	−0·5
	18. 11 5	...	...	...	...	...	...	...	...	...	...	...
	24. 14 40	...	...	...	...	...	...	...	...	...	...	...
XLII.	Apr. 24. 9 30	...	...	...	...	...	...	...	11·0	8·3	...	...
	24. 10 0	...	...	...	...	...	...	...	9·5	7·0	...	...
	24. 10 30	...	...	...	...	...	...	...	11·6	6·3	...	...
	24. 12 40	...	...	...	...	...	...	...	...	...	...	...
	24. 13 40	...	...	...	...	...	...	...	...	...	...	...
	25. 15 40	...	...	...	...	...	...	...	...	...	...	...
XLIII.	Apr. 1. 11 0	−1·3	−1·7	...	...	...	...	...	10·2	9·5	−0·3	−0·3
	8. 9 0	−2·0	...	...	...	...	...	...	...	...	...	...
	9. 7 30	−1·2	...	...	...	...	...	...	13·2	8·6	...	...
	24. 9 0	1·3	...	...	3·5	...	...	...	11·5	7·5	...	...
	24. 11 0	1·4	...	...	−0·3	...	...	...	11·2	7·7	...	...
	24. 11 30	1·6	...	...	−0·4	...	...	...	11·1	9·1	...	...
	24. 12 0	1·7	...	...	−0·8	...	...	...	10·2	8·2	...	...
XLIV.	Apr. 8. 7 20	...	2·2	...	...	...	...	...	16·2	...	...	...
	8. 8 0	...	...	...	...	...	...	...	...	...	...	...

Number of the Tables.	1844. Day, hour and minute.	Above grass in feet.							On lead.			
		1.	2.	4.	6.	8.	10.	12.	On grass.	Six inches high.	One foot high.	Three feet high.
XLI.	Mar. 23. 7 45	...	−0·1	−0·1	...	−0·3	...	...	9·6	...	...	...
	Apr. 3. 7 0	...	−0·4	−0·5	...	0·0	...	−0·1	10·3	...	...	...
	6. 7 25	...	1·9	−1·2	...	−1·2	...	−1·2	5·1	...	...	...
	9. 7 50	...	−0·4	−0·5	...	−3·6	...	...	9·3	...	...	...
	17. 11 0	...	−0·6	−1·0	...	−1·5	...	−2·0	10·3	...	5·7	...
	18. 11 5	...	1·7	1·3	...	−0·3	...	−0·2	9·9	...	...	...
	24. 14 40	...	2·0	2·0	...	1·5	...	1·5	10·0	...	...	...
XLII.	Apr. 24. 9 30	...	1·7	1·2	...	0·7	...	0·5	10·5	...	7·3	...
	24. 10 0	...	2·0	1·8	...	1·3	...	1·2	11·5	...	8·0	...
	24. 10 30	...	0·9	0·6	...	0·6	...	0·5	12·8	...	8·3	...
	24. 12 40	...	1·9	1·5	...	0·9	...	0·6	12·1	...	7·5	...
	24. 13 40	...	1·2	1·1	...	1·0	...	0·6	12·8	...	6·0	...
	25. 15 40	...	2·5	0·9	...	0·8	...	...	13·2	...	8·1	...
XLIII.	Apr. 1. 11 0	...	2·7	2·5	...	0·7	...	0·7	11·5	...	7·5	...
	8. 9 0	...	−1·7	...	...	−3·7	...	−3·7	11·5	...	...	...
	9. 7 30	...	−0·9	...	...	−3·2	...	...	10·3	...	...	...
	24. 9 0	...	1·7	...	...	1·0	...	0·8	9·7	...	7·0	...
	24. 11 0	...	1·6	...	...	0·9	...	0·9	12·2	...	9·7	...
	24. 11 30	...	1·6	...	...	0·8	...	0·8	12·1	...	9·6	...
	24. 12 0	...	1·9	...	...	1·5	...	1·2	9·9	...	3·7	...
XLIV.	Apr. 8. 7 20	...	1·4	2·0	...	1·2	...	0·5	14·7	...	12·2	...
	8. 8 0	...	...	...	...	−3·5	...	−3·5	13·0	...	...	...

Number of the Tables.	1844. Day, hour and minute.	On zinc.					On copper on grass.	On iron on grass.	On tin.		
		On grass.	Six inches high.	One foot high.	Three feet high.	Four feet high.			White on grass.	White one inch high.	Blackened on grass.
XLI.	Mar. 23. 7 45	...	...	...	...	...	10·4	7·4	...	7·9	9·6
	Apr. 3. 7 0	...	...	...	...	...	...	...	8·1	...	...
	6. 7 25	...	...	...	...	...	10·9	...	9·6	...	...
	9. 7 50	...	...	...	...	...	13·3	8·8	10·3	...	...
	17. 11 0	...	...	...	...	...	11·0	5·5	10·0	...	...
	18. 11 5	4·5	...	...	...	...	12·7	...	13·0	...	...
	24. 14 40	11·7	6·8	...	...	...	10·0	10·2	...	...	...
XLII.	Apr. 24. 9 30	13·0	8·0	5·3	5·0	...	14·0	12·5	...	...	...
	24. 10 0	13·7	9·0	5·5	4·5	...	13·3	12·5	...	...	...
	24. 10 30	12·1	7·9	4·8	3·3	...	12·8	12·6	...	...	...
	24. 12 40	12·9	7·9	...	...	...	13·1	12·8	...	...	...
	24. 13 40	13·0	6·6	...	...	...	12·9	12·1	...	...	...
	25. 15 40	10·5	6·0	...	...	...	12·0	...	...	...	...
XLIII.	Apr. 1. 11 0	5·2	...	...	...	...	13·2	8·2	7·7	10·7	...
	8. 9 0	...	...	...	...	...	14·6	...	...	...	...
	9. 7 30	...	...	...	...	...	14·4	9·8	...	...	...
	24. 9 0	13·5	9·5	...	...	...	12·0	12·7	...	...	...
	24. 11 0	12·7	7·7	4·7	4·9	...	13·7	12·9	...	...	...
	24. 11 30	12·9	8·1	4·4	4·4	...	13·3	12·8	...	...	...
	24. 12 0	13·2	7·4	4·7	4·9	...	13·9	12·2	...	...	...
XLIV.	Apr. 8. 7 20	17·7	...	...	...	...	17·5	14·5	...	...	13·7
	8. 8 0	...	...	...	...	...	...	...	...	...	...

Number of the Tables.	1844. Day, hour and minute.	On glass on grass.	On glass one inch high.	On pantile.	On slate.	On hare-skin.	On rabbit-skin.	On stone.		
								Firestone.	Purbeck.	Portland.
XLI.	Mar. 23. 7 45	10·8	...	...	...	13·6	12·9	...	...	...
	Apr. 3. 7 0	10·5	8·1	...	...	13·1	16·1	...	...	...
	6. 7 25	9·8	9·6	...	...	...	...	...	...	...
	9. 7 50	12·3	10·3	...	...	15·3	13·9	...	...	...
	17. 11 0	12·0	10·0	...	...	11·5	11·5	...	...	...
	18. 11 5	12·5	13·0	...	...	...	...	...	...	...
	24. 14 40	...	...	...	...	13·0	...	...	...	...
XLII.	Apr. 24. 9 30	13·3	11·5	...	...	15·5	15·5	...	...	...
	24. 10 0	13·0	11·5	...	...	15·0	15·0	...	...	...
	24. 10 30	12·0	11·6	...	...	15·0	15·0	...	...	...
	24. 12 40	...	...	...	...	16·1	...	...	...	...
	24. 13 40	...	...	...	...	16·3	...	...	...	...
	25. 15 40	...	...	...	...	14·3	...	...	...	...
XLIII.	Apr. 1. 11 0	13·9	11·9	...	...	13·3	13·2	...	...	...
	8. 9 0	...	12·3	...	...	14·3	11·7	...	...	...
	9. 7 30	15·0	11·6	...	...	11·8	10·6	...	...	...
	24. 9 0	13·5	12·3	...	...	14·5	14·5	...	...	...
	24. 11 0	10·7	10·2	...	...	14·7	14·7	...	...	...
	24. 11 30	13·6	12·1	...	...	15·1	15·1	...	...	...
	24. 12 0	12·2	10·7	...	...	15·9	15·7	...	...	...
XLIV.	Apr. 8. 7 20	18·7	16·7	...	...	...	...	...	...	...
	8. 8 0	...	...	...	...	16·4	...	...	...	...

Number of the Tables.	1844. Day, hour and minute.	On Lamb's wool on grass.									Clouds.			Wind.		Haze, fog, mist, or vapour.	1844. Day, hour and minute.	Number of the Tables.
		Yellow.	White.	Jet black.	Crimson.	Orange.	Light blue.	Dark blue.	Green.	Scarlet.	Modification.	Amount 0—10.	High or low.	Direction.	Strength 0—6.		h m	
XLI.	Mar. 23. 7 45	...	...	...	...	...	...	...	...	...	Clear	...	...	c.	...	Haze	Mar. 23. 7 45	XLI.
	Apr. 3. 7 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Apr. 3. 7 0	
	6. 7 25	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			6. 7 25	
	9. 7 50	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			9. 7 50	
	17. 11 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			17. 11 0	
	18. 11 5	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			18. 11 5	
	24. 14 40	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 14 40	
XLII.	Apr. 24. 9 30	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Apr. 24. 9 30	XLII.
	24. 10 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 10 0	
	24. 10 30	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 10 30	
	24. 12 40	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 12 40	
	24. 13 40	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 13 40	
	25. 15 40	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			25. 15 40	
XLIII.	Apr. 1. 11 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Apr. 1. 11 0	XLIII.
	8. 9 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			8. 9 0	
	9. 7 30	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			9. 7 30	
	24. 9 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 9 0	
	24. 11 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 11 0	
	24. 11 30	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 11 30	
	24. 12 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			24. 12 0	
XLIV.	Apr. 8. 7 20	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			Apr. 8. 7 20	XLIV.
	8. 8 0	...	...	...	...	...	...	...	...	...	Clear	...	...	c.			8. 8 0	

From the preceding tables we learn the following particulars:—Tables I. to VI., XVII. to XX., and XXIX. to XXXII. contain the results of observations when the excess of air-temperature above that of long grass was less than 5°; such observations having been taken at times when the sky was wholly covered with cirrostratus cloud, after having been cloudless, during which state a series of observations had been taken. By examining the last column of these Tables, it will be seen that when the excess of air-temperature above that of long grass has amounted to 4° or 5°, the clouds have been noted as being high, and when the excess was less than 3°, they have been mentioned as being low, or no mention has been made as to their height. Tables VII. to X., XXI. to XXIV., and XXXIII. to XXXVI. contain the results of observations at times when the excess amounted to 5°, 6°, 7°, or 8°; and from the last columns in those tables, it appears that at such times the sky was frequently cloudy, and when wholly so, that the clouds were high; that the air was in frequent motion, and at times the wind was blowing with a pressure of 4 lbs. on the square foot; that the atmosphere was occasionally thick by haze or vapour, and that dew was seldom deposited. Tables XI. to XIV., XXV. to XXVII., XXXVII. to XL. contain the results of the observations at times when the excess was 9°, 11°, or 12°; and at those times the sky has been generally clear, the air calm, and the atmosphere frequently hazy, misty, or vapour or fog was prevalent. Dew was also frequently deposited; in two cases contained in Tables XI. and XXXVII. occurring 1843, Nov. 12^d 13^h 5^m, and in 1844, Feb. 20^d 5^h, the sky was wholly covered by high and thin clouds. Tables XV. and XVI., XXVIII., XLI. and XLII., exhibit the results when the excess was 13° and 14°, and at those times the sky was generally clear, and the air was calm; haze and vapour were occasionally prevalent. Tables XLIII. and XLIV. contain the results when the air-temperature exceeded that of long grass by quantities varying from 15° to 19°; and by examining the last column of these Tables, it appears that at those times the sky was cloudless and bright, the atmosphere was clear without haze, mist or vapour, and a perfect calm prevailed.

From the above particulars it appears that at times when the sky was entirely covered with low cirrostratus clouds, the readings of a thermometer placed on long grass was the same as that in the air; that with the same clouds at a moderate elevation, the reading of the thermometer in air has exceeded that on long grass by 3°; and on those clouds being high, this excess has amounted frequently to 5°; and if other clouds than cirrostratus covered the whole sky this excess has been as large as 10°. At times when the sky has been free from clouds but not bright, haze and vapour being prevalent, the above excess has amounted to 10°, 11°, or 12°; and at times when the sky has been both bright and clear, with the air calm, no mist, haze, vapour or fog being prevalent, this difference has frequently amounted to 14°, less frequently to 19°, and sometimes to 20°.

In Tables XLII. to XLIV. are exhibited the great excess of the reading of a thermometer in air above those of thermometers placed on substances whose power for

conducting heat was bad, such as raw wool, flax, hare-skin and rabbit-skin, and contrary to expectation, even on metals, whose power for conducting heat is good.

In Table XLIV. the reading of a thermometer on raw wool was 25° less, whilst another placed at 8 feet from the ground and fully exposed to the sky, was 3°·5 greater than that in air at the height of 4 feet and protected from radiation, and thus a difference of 28°·5 existed between the readings of two thermometers, the one placed on raw wool, and the other in air at the height of 8 feet. This difference was the greatest that I have ever seen, and it occurred in 1844, on April 8^{d}, at 8^{h}.

The general agreement in the relative radiating powers of the different substances on different nights, and many other particulars, will be best seen by consulting Tables I. to XLIV.

I shall now proceed to explain the formation of the following table of the mean results derived from all the observations.

The mean of the numbers was taken in every group of results contained in Tables I. to XLV., for every different substance or different position of the thermometer in each period of observation, thus forming three large tables; from these a fourth table was formed by combining the mean results for every substance, according to the number of observations from which each had been deduced, omitting, however, all those which had been taken when the excess of air-temperature above that of long grass temperature was less than 2°*. From the numbers in this last table the next table was formed.

The last column but two in the following table contains the mean excess of the reading of the thermometer in air above those placed as stated in the first column, derived from all the observations made on each substance, &c. These numbers are smaller than they would have been had the observations been made in a wide and open plain (see introductory remarks), and also if the thermometer in the air had been perfectly protected from the effects of radiation (see remarks following Table XLV.). The numbers in the last column but one represent the relative radiating power of the several substances, that of long grass being considered as 1000. It is probable that these numbers are very accurate, for had the results in the preceding columns been larger than they are, they would have been relatively so, consequently the numbers in this column would not have been affected.

In most cases the experiments have been sufficiently numerous to give results worthy of entire confidence, the numbers in this column having been deduced from upwards of 10,000 experiments.

* These four Tables are in MSS. and placed with the series of observations.

Table XLV.

The mean excess of the reading of a thermometer placed in air at the height of four feet above the soil and protected from the effects of radiation, above those of thermometers placed on different substances, or in different situations, fully exposed to the sky.

Situation of the thermometer, its bulb, or the substance under which it was placed, being fully exposed to the sky.	Whole number of comparisons.	Whole sum of the excesses of the readings of the thermometer in air at the height of four feet, above those placed as stated in the first column.	Mean excess of the reading of the thermometer in air.	Relative excess, that of long grass being represented by 1000, or comparative radiating power.	Remarks.
On long grass	479	4017·7	8·39	1000	
On short grass	473	3454·7	7·30	870	
One inch below the surface of ground under grass	273	– 476·1	– 1·75	– 209	
On surface of ground under long grass	156	85·0	0·55	66	
On surface of ground under short grass	259	437·3	1·69	200	
On long grass covered by white raw wool	66	11·9	0·18	22	
On long grass covered by flax	61	20·4	0·35	42	
On long grass covered by white tin	35	119·3	3·41	406	
On long grass covered by white tin one inch high	42	126·3	3·00	357	
On long grass covered by blackened tin	28	131·9	4·71	561	
On long grass covered by lead	10	46·7	4·67	556	These observations were generally simultaneous.
On long grass covered by lead six inches high	13	40·1	3·08	367	
On long grass covered by glass	127	727·9	6·01	716	These observations were generally simultaneous.
On long grass covered by glass one inch high	128	557·5	3·35	399	
On long grass covered by hare-skin	18	13·6	0·75	89	These observations were simultaneous.
On long grass covered by rabbit-skin	17	10·1	0·60	72	
One inch high above the top of grass (in air)	226	1272·9	5·63	671	These factors, multiplied into the mean radiating power of any substance, will show the cooling effect of that substance upon the air, at that distance above it corresponding to the factor used.
Two inches high above the top of grass (in air)	9	43·0	4·78	570	
Three inches high above the top of grass (in air)	89	336·1	4·00	477	
Six inches high above the top of grass (in air)	165	394·1	2·37	282	
One foot high above the top of grass (in air)	212	229·4	1·08	129	
Two feet high above the top of grass (in air)	199	143·3	0·72	86	
Four feet high above the top of grass (in air)	87	50·3	0·58	69	
Six feet high above the top of grass (in air)	18	7·9	0·44	52	
Eight feet high above the top of grass (in air)	96	13·4	0·14	17	See simultaneous observations at 8 feet and 10 feet.
Ten feet high above the top of grass (in air)	9	80·0	0·89		
Twelve feet high above the top of grass (in air)	77	9·0	0·12	14	
On black-lead in powder on the raised board	138	808·1	5·85	697	
On charcoal in powder on the raised board	153	996·2	6·51	776	
On whiting in powder on the raised board	67	465·5	6·94	827	
On chalk in powder on the raised board	21	142·1	7·05	840	
On lamp-black in powder on the raised board	14	113·0	8·06	961	
On unwrought white cotton wool on grass	228	2074·6	9·10	1085	See simultaneous observations.
On unwrought white cotton wool on the raised board	15	127·7	8·52		
On white raw wool on grass	357	3655·1	10·24	1222	
On flax on grass	461	4563·6	9·90	1186	See simultaneous observations.
On flax on the raised board	17	76·3	6·94		
On yellow cotton, jeweller's wool, on grass	13	84·1	6·46	770	None of these observations were simultaneous.
On yellow cotton, jeweller's wool, on the raised board	56	472·0	8·43	1005	
On blue cotton, jeweller's wool, on grass	12	84·6	7·05	840	None of these observations were simultaneous.
On blue cotton, jeweller's wool, on the raised board	55	460·3	8·37	997	
On white wadding on grass	27	223·1	8·27	986	
On black wadding on grass	20	166·6	8·33	993	
On flannel on grass	29	212·1	7·31	871	None of these observations were simultaneous.
On flannel on raised board	55	408·7	7·43	886	
On raw silk on grass	32	254·0	7·84	934	None of these observations were simultaneous.
On raw silk on the raised board	50	464·3	9·29	1107	
On silk from the cocoon	24	208·1	8·67	1034	
On jet black lambs' wool	33	205·4	6·22	741	These observations were always simultaneous.
On white lambs' wool	33	227·5	6·89	821	
On green lambs' wool	33	220·2	6·76	806	
On light blue lambs' wool	33	242·6	7·35	876	
On dark blue lambs' wool	33	230·2	6·98	832	

The observations in air at the height of ten feet above grass, were made on one night only, viz. on 1844 April 24, and the readings were between those of eight feet and twelve feet.

No certain difference appeared from many simultaneous observations of fine and coarse flax, and consequently their results have been combined in the formation of the final values for flax.

TABLE (Continued).

Situation of the thermometer, its bulb, or the substance under which it was placed, being fully exposed to the sky.	Whole number of comparisons.	Whole sum of the excesses of the readings of the thermometer in air at the height of 4 feet, above those placed as stated in the first column.	Mean excess of the reading of the thermometer in air.	Relative excess, that of long grass being represented by 1000, or comparative radiating power.	Remarks.
On orange lambs' wool	33	233·3	7·07	844	These observations were always simultaneous.
On yellow lambs' wool	33	239·5	7·26	855	
On red lambs' wool	33	223·3	6·77	807	
On crimson lambs' wool	33	225·3	6·83	814	
On garden mould	151	598·5	3·96	472	These observations were generally simultaneous.
On gravel	149	361·4	2·42	288	
On river sand	157	599·0	3·81	454	See simultaneous observations.
On river sand on the raised board	25	127·6	5·10		
On tinfoil	33	129·8	3·94	470	
On lead	224	1421·7	6·35	757	See simultaneous observations.
On lead six inches high	13	53·2	4·10		
On lead one foot high	48	261·3	5·44		
On lead three feet high	2	10·5	5·25		
On copper	172	1210·5	7·04	839	
On iron	161	868·2	5·39	642	See simultaneous observations.
On zinc	213	1217·9	5·71	681	
On zinc six inches high	29	175·2	6·04		
On zinc one foot high	6	29·4	4·90		
On zinc three feet high	6	27·0	4·50		
On zinc four feet high	4	13·2	3·30		
On white tin	112	616·6	5·51	657	
On white tin one inch high	75	378·0	5·04	601	Generally simultaneous observations.
On blackened tin	104	672·2	6·46	770	
In focus of metallic parabolic reflector	464	3340·7	7·20	858	
On the raised board	50	323·4	6·49	773	
On saw-dust in a box on the raised board	37	189·3	5·12	610	
Nine inches above wood and protected from lateral wind	394	1236·8	3·11	371	
On brick	46	143·5	3·12	372	
On pantile	111	437·2	3·94	470	
On slate	100	481·1	4·81	573	
On glass	163	1182·3	7·25	864	These observations were generally simultaneous, and with them the observation of grass under the glass.
On glass one inch high	156	954·1	6·11	728	
One quarter of an inch above water	83	225·7	2·72	324	
On paper on the raised board	8	41·2	5·15	614	
On hare-skin	70	773·1	11·04	1316	
On rabbit-skin	59	613·2	10·40	1240	See simultaneous observations.
On stone	154	503·3	3·27	390	
Number of observations of the thermometer in air, and its mean reading	487		43·7		

No certain difference was found from many simultaneous experiments on lead less than the twentieth of an inch in thickness, and lead a quarter of an inch in thickness; the observations are everywhere used as lead, independently of its thickness. The same remark applies to the three different thicknesses of zinc, and to the three different stones used; and their results have been combined as one result for "zinc," and as one result for "stones" in the final result.

Some of the particulars which we may learn from the preceding table are the following.

The first results contained in it are those relating to grass, whose radiating power appears to be such, that the reading of a thermometer when placed on it when long, is less than when it is placed on short by 1°·1; the next result relating to grass is that of the temperature on the surface of the soil under it, which is such that the reading of a thermometer under long, exceeds that under short by 1°·1; being exactly the same amount in excess under as it was in defect on the top; and hence the cause of the difference of the readings on the top of long and short grass arises solely from the greater quantity of heat conducted to the surface of the latter from

the soil, over that conducted to the surface of the former, and not from the greater quantity of heat radiated into space from the long, over that radiated from the short; such being the case, it was to be expected that the readings of a thermometer would vary with every variation of the length of grass upon which it was placed, and such was found to be the case.

In fact, the readings of thermometers placed on grass were found to differ with every variation of length, fineness and closeness of its blades. My experiments have been made on that differing only in the length of its blades, and the differences arising from this cause were found to vary with every variation of the excess of the temperature of the air over that of long grass. The following are the mean results of experiments in this respect:—

When the excess of air-temperature at the height of 4 feet above that of long grass was	between 3° and 7°	the reading of the thermometer on short grass was higher than that on long grass by	0°·8 from 125 comparisons.
	between 7° and 10°		1°·3 from 231 comparisons.
	between 10° and 19°		1°·9 from 86 comparisons.

These differences were found to correspond to a mean temperature of the air of 44°, and that of long grass of 36°, or generally to all temperatures above 30°; but when the reading of long grass declined below 30°, that of short grass was found not to decline nearly so rapidly: investigating the temperatures at readings below 30°, the following are the mean results:—

That when the reading on long grass was between	25° and 30°	it exceeded that on short grass by ...	2°·0 from 10	Comparisons.
	20 and 25		2·4 from 31	
	15 and 20		3·3 from 19	
	10 and 15		4·8 from 14	
	5 and 10		6·4 from 9	
	0 and 5		9·4 from 2	

From another investigation, it appeared that these differences were connected with the difference between the readings of the thermometer in air and that on long grass as follows:—

When the reading of the thermometer on long grass was	at and above 30°	the excess of the reading of the thermometer on short grass was	one-eighth part,	of the excess of the reading of the thermometer in air at 4 feet high, above that on long grass.
	at 20°		one-fourth part,	
	at 10°		one-half,	
	at 0°		the whole,	

Hence it appears that at the reading of 0° of the thermometer on long grass, the reading of a thermometer on short grass is as much higher than 0°, as the reading of the thermometer in air at the height of 4 feet is higher than 0°; or that the readings on short grass and that in air are alike, and therefore that the heat conducted from the earth equals in amount the heat lost by radiation; and that for readings on long grass increasing from 0° in an arithmetical progression with a common difference of 10°, the difference between the readings on long and on short grass becomes less in geometrical progression with a common ratio of one-half; till attaining a reading of 30° the difference equals the fourth term of the series, and this difference continues very nearly constant till 60°, above which temperature I have had but few experiments. If we call the excess of air-temperature above that of long grass temperature by e, then the excess of the readings on short grass above those on long grass at

0°, 10°, 20° and at 30°, and above 30°, will be respectively e, $\frac{1}{2}\,e$, $\left(\frac{1}{2}\right)^2 e$, $\left(\frac{1}{2}\right)^3 e$; and for any intermediate reading the difference can be easily calculated from these terms. The very singular and unexpected facts now detailed, merit attention and suggest the necessity of carefully noting the position of a thermometer in any investigation in which such instrument is needed, as indeed do all the experiments that I have made, though not in so marked a manner as does this. I now proceed to the results of those experiments in which grass was covered by different substances, which are as follows:—

The covering of long grass by					
raw wool, flax, hare-skin or rabbit-skin	caused the thermometer to read higher than one placed on uncovered grass by	the whole	of the difference between the thermometer in air at the height of 4 feet and that on long grass	or in its mean, caused the covered grass to be warmer by.........	8·0
a sheet of white tin ...	,, ...	five-eighths	... ,, ...	... ,, ...	5·0
a sheet of white tin one inch high ...	,, ...	five-eighths	... ,, ...	... ,, ...	5·4
a sheet of blackened tin	,, ...	three-eighths	... ,, ...	... ,, ...	3·6
a sheet of lead...... ...	,, ...	three-eighths	... ,, ...	... ,, ...	3·6
a sheet of lead 6 inches high.........	,, ...	five-eighths	... ,, ...	... ,, ...	5·4
a sheet of colourless glass...............	,, ...	one-fourth ...	... ,, ...	... ,, ...	2·4
a sheet of colourless glass 1 inch high	,, ...	five-eighths	... ,, ...	... ,, ...	5·0

The following are some of the particulars that we may collect from these results. The filamentous substances and skins did not allow any heat to escape from the grass they covered, thus proving them to be very bad conductors of heat. The raising any substance above the grass caused the thermometer reading to increase, although in this case it must have been exposed to a portion of the sky: this in the case of tin was a small quantity; in the cases of lead and of glass the quantity was large, with the former amounting to 1°·7, and with the latter to 2°·7 in their means. The amount of heat transmitted through a colourless medium, as glass, is remarkable, the mean amount of radiation from grass thus covered being less by only one-fourth part of that of uncovered grass.

The next investigation connected with grass was that of placing thermometers at different distances from it, with the view of determining the cooling effect of a body, as cooled by radiation, upon the air in contact with it; the results of these experiments as contained in the table, will probably be more clearly seen by arranging the numbers on the supposition that the reading of a thermometer on long grass was 0°, and combining with them those of 1 inch below and on the surface of the soil as follows:—

At one inch below the surface of the soil the mean reading would be	10·24
On the surface of the soil under long grass the mean reading would be . .	7·84
On long grass fully exposed to the sky the mean reading would be	0·00
One inch above long grass fully exposed to the sky the mean reading would be	2·76
Two inches above long grass fully exposed to the sky the mean reading would be	3·61

Three inches above long grass fully exposed to the sky the mean reading would be 4°·39
Six inches above long grass fully exposed to the sky the mean reading would be 6·02
One foot above long grass fully exposed to the sky the mean reading would be 7·31
Two feet above long grass fully exposed to the sky the mean reading would be 7·67
Four feet above long grass fully exposed to the sky the mean reading would be 7·81
Six feet above long grass fully exposed to the sky the mean reading would be 7·96
Eight feet above long grass fully exposed to the sky the mean reading would be 8·26
Twelve feet above long grass fully exposed to the sky the mean reading would be 8·27
And the one in air at the height of four feet, protected from six-tenths of the sky, would be . . . . 8·39

The numbers in the last column of Table XLV., opposite to the respective heights above long grass, are factors deduced by considering the radiation from long grass to be represented by unity, and therefore the numbers represent the factors corresponding to the cooling effect of a body cooled by radiation upon the air at different heights above it. They may be considered to apply to all bodies whose radiating powers are known, the cooling effect of which upon the air at any particular distance above them, less than twelve feet, will be known by multiplying the mean radiating power into the factor corresponding to that distance.

The greater coldness of grass than that of the air in clear and calm weather, in places sheltered from the sun but open to a considerable portion of the sky, may continue all the day as well as night, and from many unrecorded observations, this appears to occur frequently. On September 22[d], 1843, this was observed to be the case; the day was fine, the sky was nearly cloudless, and the reading of the barometer was high. A thermometer on the grass was placed in the shade, and exposed to as much of the sky as possible, and the following observations were taken:—

1843. Month, day, and hour.	Reading of thermometer		Excess of reading of thermometer in air above that on grass.	Remarks.
	In air.	On the grass.		
h m				
Sept. 21. 21 30	62°·8	58°·5	4°·3	Cloudless; dew abundant.
22 15	60·0	54·3	5·7	Cloudless; dew in globules on grass.
23 20	65·5	64·0	1·5	Large clouds to the north; dew abundant.
23 50	66·5	61·5	5·0	Cloudless; dew abundant.
Sept. 22. 0 0	69·0	62·0	7·0	A few clouds to the north.
1 0	68·9	61·5	7·4	Thin clouds to the north.
1 40	68·7	59·0	9·7	Cloudless. The thermometer was frequently moved so as to keep it in the shade, and on grass upon which the sun had not shone for a long time.
1 53	68·7	58·8	9·9	Cloudless.
3 30	67·0	58·4	8·6	Cloudless.
4 0	66·6	59·4	7·2	Cloudless.
4 20	66·3	58·7	7·6	Cloudless.
4 35	65·8	57·5	8·3	Cloudless.
5 0	65·2	56·8	8·4	Cloudless.

At noon on this day a thermometer was placed on grass upon which the sun had been shining till Sept. 21^d, 23^h 45^m, and the following observations were taken :—

1843. Month, day, and hour.	Reading of thermometer In air.	Reading of thermometer On the grass.	Excess of reading of thermometer in air above that on grass.	Remarks.
Sept. 22. 0h 0m	69°·0	67°·0	2°·0	A few clouds to the north.
1 0	68·9	64·5	4·4	Thin clouds to the north.
1 40	68·7	64·0	4·7	Cloudless.
3 30	67·0	61·7	5·3	Cloudless.
4 0	66·6	61·5	5·1	Cloudless.
4 20	66·3	61·0	5·3	Cloudless.
4 35	65·8	59·7	6·1	Cloudless.
5 0	65·2	59·0	6·2	Cloudless.

During the whole of this day the temperature of the grass in the shade was several degrees below that of the air at four feet above it, and dew remained all day upon that portion of grass upon which the sun had not shone. Also the temperature of that upon which the sun had been shining soon began to decline, but it did not descend so low as that of the dew-point, which was about 58°, and therefore no dew was deposited upon it.

This day followed a long period of hot weather without rain, and the day was nearly cloudless, with the sun shining brightly.

This closes the experiments connected with grass with mercurial thermometers, and it will be seen that I have paid very much attention to them, as grass is a substance upon which many former experiments have been made, the discrepancies between which are now fully accounted for.

I may here point out one important result of the preceding experiments; viz. the amount of the corrections dependent upon the portion of clear sky to which the bulb of a thermometer is exposed, necessary to be applied to its readings to obtain from them the true temperature of the air. As a thermometer at the height of four feet, protected from six-tenths of the sky, was found to read 0°·58 higher than another at the same height fully exposed to the sky, it follows that the effect of protecting the bulb of a thermometer from the sky, whose height from the ground was four feet, was to cause the readings to increase 0°·1 for every tenth part of the sky protected: hence, if a thermometer be wholly exposed to the sky, it is necessary to increase its readings by

$$0°\cdot 1 \times \text{number of tenths of cloudless sky};$$

and if the bulb be partially protected from the sky, the correction is

$$\left(10 - \left\{\begin{matrix}\text{quantity of sky in tenths from} \\ \text{which the bulb is protected}\end{matrix}\right\}\right) \times 0°\cdot 1 \times \text{number of tenths of clear sky.}$$

From this it appears that the thermometer, which throughout all the experiments has been considered to have been protected from the effects of radiation, was yet subjected to it to the amount of 0°·4 on a cloudless sky, or to 0°·4 × number of tenths of clear sky at times when the sky was partially cloudy.

If attention be paid to the placing of thermometers, so that their bulbs be in the shade, protected from rain and from the effects of radiation from walls (by placing their bulbs at least six inches from them), but in other respects freely exposed to the air, and correcting their readings by the preceding formulæ, it will ensure the obtaining the true temperature of the air from them at night. To avoid the effects of reflected heat during the day, the thermometers should be placed at a greater distance than six inches from walls, &c., and their readings corrected as before by the preceding formulæ. No simple rules have hitherto been given for placing a thermometer so that from its readings the true temperature of the air could be deduced.

The readings of thermometers placed on grass being found so variable, the unfitness of it to furnish the means of comparing the degrees of cold at night on the surface of the earth was evident. A much greater uniformity was observed in the results of experiments made with other substances, which were bad conductors of heat, and whose condition was always the same.

Of all the substances experimented upon, those on which the readings of the thermometers have been the lowest were hare-skin and rabbit-skin*; and upon the filamentous, as raw wool, flax, unwrought cotton wool, and raw silk, the readings upon all of which were more steadily less, on clear and calm nights, and exhibited a greater degree of cold, than those on grass. Among bodies of this class, raw wool exhibited a lower reading at all times than any of the rest; the circumstance of hare-skin and rabbit-skin exhibiting a lower mean in the table is accidental, experiments on these substances having been made only during the finest nights, and on which nights raw wool exhibited lower readings than they did. The next in order is flax, both fine and coarse, and in its mean it exhibits a radiating power very nearly equal to that of raw wool; but the last-mentioned substance was much more sensible on the approach of a cloud than any other substance: on those nights which were clear for many hours, raw wool obtained its lowest readings long before flax; and the readings on wool, on the approach of a cloud, increased much sooner than those on flax; and in consequence of the latter circumstance, the mean values deduced from raw wool and flax were nearly the same. In intervals of clear sky, between cloudy states of it, the difference between the readings of two thermometers, the one in air and the other on raw wool, was the greatest of any of the differences. The rapidity of the decrease of the readings on raw wool from a cloudy to a clear sky was about a degree per minute, so that a change in the readings of 15° has taken place in a quarter of an hour. The greatest difference between the temperature of a body at night on the surface of the earth, and that of air a few feet above the earth, was 28°·5; this extraordinary difference occurred in 1844, April 8^d, at 8^h (See Table XLIV.). The times

* Thus explaining the fact frequently noticed by sportsmen, that the snow upon which hares have been lying is never in the slightest degree melted.

at which other large differences took place, with their amounts, will be seen in Section II.

The next class of bodies consisted of black and white wadding, flannel, and wool of different kinds and colours. Of these, black and white wadding exhibited the lowest readings, and were nearly equal in amount; flannel was the next in order, and the various-coloured lambs' wool were the next; but the differences between those bodies with respect to their radiating powers were not large; they were all, however, inferior in their radiating powers to bodies of the first class. It will be seen in Table XLV. that the numbers opposite to flannel, yellow and blue cotton wool, when those substances were placed on the raised board, are larger than those numbers with the same substances placed upon grass; these differences most probably arose from the different quantities of heat which they received from the parts beneath, the several substances being slow conductors of heat.

The observations upon the coloured wools were too few to indicate positively the influence of colour, nevertheless they were all good and simultaneous experiments. The order of their radiating power is black the lowest, then green, white, crimson, scarlet, orange, yellow, dark blue and light blue successively, the difference between light blue and black being 1°·3. The same parcels of wool were exposed to the direct rays of the sun many times, and as nearly similarly situated as possible. The mean of fifty-eight simultaneous readings of the thermometers placed on them gave—

Black	105°
Yellow	105
Scarlet	107
Orange	109
White	110
Green	110
Crimson	110
Dark blue	110
Light blue	121

Thus black and light blue are at the extremes of the absorptive powers, and so far confirm the results obtained by the night observations, as the absorptive power of substances is proportional to their radiating power.

But as these results with respect to colours may have been affected with the particular parcels of wool I used, and different results might have been obtained had other parcels been used, I do not place much confidence in them; yet, so far as I could discover, each parcel was equal in thickness, hardness, and in fact in every respect except that of colour.

Bodies in the state of powder, placed on the raised board, formed a third class of substances. These were black-lead, charcoal, whiting, chalk, lamp-black and river-

sand. The lowest readings occurred on lamp-black; the next in order were chalk, whiting, charcoal, black-lead and sand; a quantity of the last-mentioned substance was also placed on the ground to the depth of three inches, and to an extent of nine square feet; the difference between the mean readings of thermometers placed on sand in these two positions is probably accidental, arising from the observations in the former case having been taken on very fine nights only.

Building materials formed a fourth class; these consisted of glass, stone, brick, pantile, wood and slate. The lowest readings occurred on glass, and therefore this substance exhibited the greatest radiating power; the next in order were wood, slate, pantile, brick and stone successively; the three last-named substances were nearly equal.

Metals formed a fifth class; these consisted of tinfoil, lead, zinc, copper, iron, white tin, blackened tin, and the thermometer placed in the focus of a polished metallic reflector; of these lead and zinc of different thicknesses were experimented upon, and the same results were obtained (see Table XLVII.), from which circumstance it is inferred that the thickness of a metal has no effect on its radiating power. The metals were all placed on grass, and from the circumstance of the ready passage of heat from one part of a metal to another, much heat must have passed from the earth to their upper surface when so placed. The lowest readings of this class were those of the thermometer in the focus of the reflector; the next in order, and very nearly equal to it in amount, was copper, then blackened tin, lead, zinc, iron and white tin. Two of these metals, viz. lead and zinc, were placed at different distances from the earth, the plane of each sheet being parallel to the horizon in all cases, and in every individual experiment the same result was obtained as that exhibited in the means, viz. that a metal when raised above the ground, though only by one inch, was always warmer than one on the grass, though in the latter case, from its great conducting power for heat, some must have been received from the earth, whilst in the former case, heat only could have been derived from the air flowing past both its under as well as its upper surface.

The temperature of metals as exhibited in these results, contrary to what was expected, is much below that of the surrounding air; this is particularly the case with copper; this metal radiates heat so freely, that with respect to its amount, it would be placed in the second class of substances. Many simultaneous experiments were made with copper when placed on one of the angles of the box, within which was placed the thermometer in the parabolic reflector, with the readings of the latter thermometer; thus situated, the thermometer in the box was protected from lateral wind, whilst that on the copper was subject to the passing air; and thus situated the readings on the copper were lower than those in the reflector, except at times when the air was passing quickly.

Whilst speaking of metals, I may remark here that I never found the reading of a thermometer placed on a metal which had been moved successively from one part of

the grass plat to another, was lower than when it was placed on a similar piece of metal which had remained unmoved.

A piece of metal, if so placed that part of it be in contact with grass and the other part not, that part of it in contact with the grass will be at a lower temperature than that which is not in contact.

The thermometer whose bulb is in the focus of the mirror when the clouds are low remains stationary and frequently reads higher than any other thermometer however placed; the reason seems to be, that the heat radiated from the cloud being received on the reflector and reflected to the bulb in the focus, exceeds that radiated from the thermometer.

The remaining results in Table XLV. are from experiments on garden mould, gravel, saw-dust and paper; also from those made by placing the bulb of a thermometer nine inches above wood, and protected from lateral wind by wood one foot high, at the distance of nine inches from the bulb all round; and from those made by placing the bulb of a thermometer a quarter of an inch above water. Of these, paper exhibited the greatest cold; and from many unrecorded experiments, it always exhibited a great degree of cold. The next in order were saw-dust, garden mould, nine inches above wood, a quarter of an inch above water, and lastly gravel; this last-mentioned substance apparently exhibited the lowest radiating power of any substance contained in this class; but the circumstance of thermometers placed on garden mould and on gravel reading higher than when placed on other substances, was found to be in consequence of the observations having always been made on the ground, of which they formed a part, and the respective surfaces of which were readily supplied with heat from beneath, and thus prevented from exhibiting a great degree of cold from their situation and not from the nature of their substance. This was proved from the circumstance that small parcels of garden mould and gravel placed on the raised board, in which situation but little heat passed from the board to their surface, were found in a few nights to exhibit very low readings of thermometers placed on them. After having found this, I placed a thermometer with its bulb one inch below the surface of the garden mould, and at the same time another one inch below the surface of the ground under grass, and I found that the readings of the latter were usually some degrees higher than those of the former at times when the sky had been clear for some time, plainly indicating the cause of the higher readings on the surface of the mould to be the very ready passage of the heat from beneath to its surface; and, therefore, as before observed, arising from its situation and not from a property inherent in itself. I made no experiments on the amount of heat thus conducted to the surface of the mould and of gravel, but it evidently must be equal, or very nearly so, to the amount radiated from those substances. I made some experiments on the quantity and on the rapidity of heat conducted upwards from one inch below the surface of the ground under grass; these were made by placing thermometers with their bulbs one inch below the ground, on the surface of

the ground immediately under others placed on the top of long and short grass; all the observations thus taken have been copied out, and the difference between every consecutive pair of observations has been consulted with respect to the interval of time between them. The results of this investigation are contained in the following tables:—

Table XLVI.

Showing the quantity and the rapidity of heat conducted from one inch below the surface of the soil, to the surface and to the top of grass.

Year, month and day.	Mean reading of a thermometer in air at the height of four feet.	Long grass. Amount by which the reading of a thermometer read less when placed		Short grass.				The decrease per hour in the reading of a thermometer placed			Number of hours during which the readings were frequently examined and from which the results were deduced.	
		On the top of long grass than that in air at the height of four feet.	On the top of long grass than that on the surface of ground under long grass.	On the top of short grass than that in air at the height of four feet.	On the top of short grass than that under short grass.	Under short grass than that one inch beneath the surface of ground.	On the top of short grass than that one inch below the surface immediately under.	On the surface of ground under long grass.	On the surface of ground under short grass.	One inch below the surface of ground under short grass.	Long grass.	Short grass.
	°	°	°	°	°	°	°	°	°	°	h	h
1843, Dec. 11.	40·0	8·2	10·0	7·4	7·2	3·7	10·9	0·5	0·8	0 6	6	5
14.	44·9	6·0	3·4	5·6	2·6	1·5	4·1	0·3	0·4	0·7	2	2
15.	45·7	5·3	3·5	5·2	1·8	1·2	3 0	0·3	1·3	0·7	13	1
15.	45·7	...	...	...	...	...	...	...	0·6	0·4	...	4
17.	42·3	7·7	7·2	6·8	4·9	2·4	7·3	0·2	0·3	0·4	10	10
1844, Jan. 6.	43·5	...	...	5·1	1·9	3·4	5·3	...	1·0	1·0	...	2
22.	35·9	10·0	7·5	8·9	7·2	1·8	9·0	1·1	1·2	0·6	2	3
26.	42·7	9·8	4·5	7·4	1·5	3·3	4·8	0·3	0·6	0·8	2	2
30.	44·9	8·4	5·5	6·6	1·5	1·4	2·9	0·5	...	1·1	1	1
Feb. 1.	31·7	7·2	9·2	5·5	6·9	2·1	9·0	0·4	0·4	0·4	6	6
15.	34·6	10·6	11·5	7·7	7·3	2·9	10·2	0·4	0·1	0·4	4	4
16.	39·6	8·7	7·0	8·0	4·9	1·2	6·1	0·7	0·5	0·8	6	5
20.	33·4	...	...	6·7	6·5	3·0	9·5	...	1·1	0·3	...	3
22.	25·8	7·5	13·7	6·1	10·9	2·1	13·0	0·3	0·2	0·2	12	12
28.	35·0	6·2	8·0	...	...	...	...	0·3	...	...	7	
March 20.	30·8	8 4	12·8	...	...	...	...	0·3	...	...	9	
21.	37·6	10·8	9·2	...	...	...	...	0·4	...	...	2	
23.	39·7	12·5	11·4	...	...	...	...	0·3	...	...	1	
April 1.	42·0	9·7	11·1	8·4	5·8	5·4	11·2	0·6	0·3	0·2	10	8
2.	49·2	9·7	8·4	8·2	5·4	3·8	9·2	1·8	1·0	1·2	4	4
3.	47·0	11·4	8·8	9·5	2·5	3·3	5·8	0·4	0·1	0·6	11	6
8.	44·9	13·0	8·3	11·2	6·8	3·4	10·2	0·6	0·4	0·8	10	7
9.	52·6	13·0	9·2	11·2	3·6	5·3	8·9	0·8	1·0	1·0	1	1
17.	46·5	9·4	7·5	9·0	4·0	5·5	9·5	0·4	0·3	0·4	6	8
18.	42·1	11·9	12·4	...	...	...	...	0·7	...	...	9	
23.	54·7	7·3	5·5	7·0	3·0	2·7	5·7	1·2	0·8	1·6	2	1
24.	45·5	15·0	13·7	12·8	5·4	7·7	13·1	1·0	1·2	0·3	3	3
25.	50·2	10·0	9·2	9·1	4·0	6·0	10 0	2·0	2·0	1·5	12	10
27.	47·0	...	...	9·0	9·9	6·3	16·2	...	2·1	2·2	...	2
29.	40·8	8·3	5·8	7·3	7·8	1·9	9·7	1·2	0·6	0·2	2	4
May 1.	46·0	11·4	11·0	9·9	5·8	6·1	11·9	1·2	1·2	1·2	10	9

By taking the mean of the numbers in each column, we find that—

The mean reading of a thermometer placed on long grass was less than that in air by . . . . 9°·5

The mean reading of a thermometer placed on short grass was less than that in air by . . . . 8·0

The mean reading of a thermometer placed on long grass was less than that under long grass by . . . . 8·7

The mean reading of a thermometer placed on short grass was less than that under short grass by . . . . 5·2°

The mean reading of a thermometer placed under short grass was less than that one inch below the surface of the ground under short grass by . . 3·5

The mean reading of a thermometer placed on short grass was less than that one inch below the surface of the ground under short grass by . . . . 8·7

The mean rate of decrease in the reading of a thermometer placed under long grass was, per hour . . . . 0·7

The mean rate of decrease in the reading of a thermometer placed under short grass was, per hour . . . . 0·8

And the mean rate of decrease in the reading of a thermometer placed one inch below the surface under short grass was, per hour . . . . 0·75

In this investigation the excess of the reading of a thermometer placed one inch below the surface of the ground under long grass, above that placed on the surface of the ground, has not been deduced; from a few experiments it appeared to be exactly the same as that of short grass; and assuming such to be the case, the mean difference on clear and calm nights between the reading of two thermometers, the one placed on long grass and the other placed one inch below the ground immediately underneath the other, was 13°·0; and the same difference with respect to short grass was 11°·5. As the results contained in this table were deduced from observations taken at times when the temperature of the long grass was above 30° generally, and occasionally a little below 30°; and as it appears from the remarks following Table XLV. that at temperatures much below 30° very great differences occurred with grass of different lengths, it was desirable to investigate observations similar to the above at low temperatures: such observations I took only on one night, viz. that of March 13, 1845; during this night the thermometers were read hourly for eight hours, and during the time—

The mean reading of the thermometer in air at the height of four feet was . 15°·1

The mean reading of the thermometer on long grass was . . . . 5·6

The mean reading of the thermometer on the surface of the soil under long grass was . . . . 26·9

The mean reading of the thermometer one inch beneath the surface of the ground under long grass was . . . . 33·1

The mean reading of the thermometer on short grass was . . . . 11·1

The mean reading of the thermometer on the surface of the ground under short grass was . . . . 21·4

The mean reading of the thermometer one inch beneath the surface of the ground under short grass was . . . . 28·2

The results of these experiments exhibit in a marked manner the badness of the conducting power of grass for heat, the reading on long grass being less than that on

the surface of the ground immediately beneath it by 21°·3, and less than that one inch below the surface by 27°·5; and the reading of that on short grass was less than that on the ground immediately beneath it by 10°·3, and less than that one inch beneath the surface of the ground by 17°·1. The mean rate of decrease of temperature at one inch beneath the surface of the ground under long grass was 0°·2 per hour, and under short grass was 0°·3 per hour; and on the surface of the ground under short grass was 0°·4 per hour. The difference between the readings at one inch beneath the surface under long grass and under short grass was 4°·9, the former being the higher by this amount; the difference between the readings on the surface of the soil under long grass and under short grass was 5°·4, that under long grass being the higher of the two; and the difference of readings on long grass and on short grass was 5°·4, that on long grass being the lower of the two; and thus the reading on long grass was as much less than that on short grass as that under long exceeded that under short: this circumstance affords a sufficient reason for the temperature of short grass being warmer than that of long grass, the heat passing so much more freely from the earth to it than in the case of long grass.

I shall conclude this part of this section, with remarking that the various amounts of dew deposited, at the same time, on different bodies at night, were found to be, as near as could be determined, proportional to the amounts of the depression of their temperature below that of the dew-point. Hence, it is evident that all hygrometers formed of any of these substances, or of any animal or vegetable substance, when exposed to the clear sky at night, will be cooled by the radiation of their heat, and will cool the air in contact with them; and thus indicate a greater degree of humidity than actually exists; and particularly so, should their temperatures descend below that of the dew-point, and dew be actually deposited upon them.

The following table contains the results of special simultaneous observations, made in some cases in consequence of the results as deduced from the ordinary observations not agreeing with each other, and in others to determine the amount of the correction due to the placing of the different substances on the raised board.

Table XLVII.—Results of Special Simultaneous Observations.

Excess of the reading of the thermometer in air at the height of four feet protected from the effects of radiation, above those of thermometers, in simultaneous observations, placed

1843. Day, hour and minute.	One inch above grass.	Two inches above grass.
Sept. h m	°	°
13. 12 0	12·0	7·5
13. 12 30	12·3	6·8
13. 13 20	11·7	5·7
13. 14 30	10·5	5·2
13. 15 15	10·3	5·3
13. 15 20	8·5	4·5
13. 15 24	6·0	3·5
13. 15 37	4·5	3·0
13. 17 20	5·5	3·5

1843. Day, hour and minute.	On white unwrought cotton wool. On grass.	On white unwrought cotton wool. On the raised board.
h m	°	°
Sept. 13. 13 30	11·7	11·8
13. 14 30	11·5	10·0
13. 15 15	12·3	10·0
13. 15 20	9·5	8·3
13. 15 24	6·0	5·5
13. 15 37	4·5	4·5
13. 17 20	5·5	5·5
Oct. 25. 5 15	13·4	14·5
25. 8 0	11·6	10·3
25. 9 30	13·0	11·7
25. 11 40	10·8	11·0
25. 13 0	9·5	10·2
25. 15 0	8·9	9·9
25. 18 15	9·0	8·8
25. 19 30	7·5	6·2
25. 21 0	6·1	2·6

1843. Day, hour and minute.	On flax. On grass.	On flax. On the raised board.
Nov. h m	°	°
7. 10 0	6·5	5·5
7. 11 10	6·5	4·3
8. 9 10	8·0	5·8
8. 11 0	8·6	6·1
11. 5 30	8·3	8·3
11. 7 30	13·2	10·2
11. 9 50	9·5	7·5
11. 10 35	10·5	7·4
11. 11 10	13·3	9·7
11. 12 0	9·5	7·5
12. 4 0	5·9	4·3

1843. Day, hour and minute.	On sand. On the ground.	On sand. On the raised board.
Sept. h m	°	°
13. 12 0	7·5	7·5
13. 12 30	7·8	8·3
13. 13 30	9·2	8·9
13. 14 30	8·5	7·5
13. 15 15	8·8	8·3
13. 15 20	7·0	5·0
13. 15 24	6·0	5·0
13. 15 37	4·5	4·0
13. 17 20	5·5	4·5
15. 7 20	3·2	2·2
15. 9 10	3·5	5·3
15. 10 33	5·0	7·3
15. 11 0	5·3	6·8
16. 7 40	4·0	3·4
16. 9 10	3·6	4·8
16. 11 20	3·4	3·5
17. 11 0	3·3	3·8
17. 11 30	4·2	4·3
17. 12 30	3·6	4·2
17. 13 20	3·4	3·3
17. 15 20	2·8	2·7
17. 17 20	2·9	2·6
18. 7 30	4·0	3·8
18. 8 0	4·1	3·9
18. 9 0	8·0	6·0

1843. Day, hour and minute.	On zinc. Thin.	On zinc. Moderately thick.	On zinc. Thick.
h m	°	°	°
Nov. 29. 12 30	6·0	4·3	5·0
29. 13 45	5·7	6·0	6·0
29. 15 0	5·3	5·5	5·8
29. 17 0	1·8	3·9	3·8
29. 18 0	2·5	3·9	3·7
29. 19 0	4·0	4·7	4·5
Dec. 2. 3 40	3·6	3·3	3·0
2. 4 30	4·7	4·5	4·0
5. 16 15	7·2	6·7	6·9
5. 17 30	5·6	5·3	5·3
5. 19 30	5·8	5·5	5·8
5. 21 0	4·5	4·7	4·3
6. 3 45	4·6	4·1	4·2
6. 4 15	5·3	5·2	5·0
6. 4 45	5·9	5·7	5·9
6. 11 0	3·2	3·2	4·2
6. 14 0	5·0	4·5	4·8
8. 4 30	9·1	8·1	9·1
8. 9 30	8·5	8·5	8·3
11. 3 55	7·0	6·8	6·8
11. 4 30	7·2	7·2	7·2
11. 5 30	5·4	6·0	6·1
11. 6 30	4·8	4·8	4·8
11. 9 30	5·6	5·2	5·2
13. 18 0	3·5	3·5	3·2
14. 3 0	3·9	3·7	3·7
14. 4 0	4·5	4·5	4·5
14. 5 0	4·2	4·2	4·2
15. 3 45	3·2	3·5	3·1
15. 5 0	4·0	3·8	3·8
17. 4 0	5·5	5·5	5·5

1843. Day, hour and minute.	On stone. Fire.	On stone. Portland.	On stone. Purbeck.
h m	°	°	°
Dec. 11. 4 30	2·2	2·4	2·7
11. 5 30	2·5	2·9	3·1
11. 6 30	3·0	4·0	3·0
11. 9 30	2·3	2·6	1·9
13. 18 0	3·0	2·5	2·0
14. 3 0	2·2	2·4	1·9
14. 4 0	2·5	2·6	2·5
14. 5 0	2·6	2·9	2·2
15. 3 45	1·9	1·7	1·7
15. 5 0	2·9	2·6	2·9
15. 21 0	3·0	3·8	2·8
17. 4 0	2·2	3·2	3·2
1844.			
Jan. 17. 5 0	4·3	3·5	3·4
26. 5 10	5·3	4·8	5·0
26. 7 30	4·7	4·1	4·5
30. 4 30	5·3	4·0	4·6
30. 5 30	3·5	2·6	3·1
Feb. 1. 3 30	2·7	2·7	2·4
1. 5 0	4·5	6·0	4·0
1. 7 0	0·5	2·0	4·2
1. 9 0	2·7	2·0	2·7
15. 15 0	4·3	4·3	4·6
15. 17 30	2·8	2·8	5·0
15. 18 30	2·5	1·5	4·5
15. 19 15	3·8	3·3	6·0
16. 4 30	4·5	4·7	5·7
16. 5 30	5·0	5·0	5·8
16. 7 0	4·4	4·7	5·4
16. 8 0	3·5	3·3	4·0
16. 9 0	4·6	4·0	5·1
20. 5 0	4·5	4·9	4·7

1844. Day, hour and minute.	On lead. On grass.	On lead. Six inches above grass.
Feb. h m	°	°
1. 5 0	8·3	4·5
1. 7 0	3·8	4·0
1. 9 0	3·7	4·7
15. 15 0	6·3	5·0
15. 17 30	7·5	4·9
15. 18 30	7·3	6·3
15. 19 15	6·5	5·0
16. 4 30	7·7	4·5
16. 5 30	10·0	7·0
16. 7 0	5·7	3·2
16. 8 0	5·5	2·5
16. 9 0	6·3	2·8
	One foot high.	Three feet high.
May h m	°	°
1. 15 0	6·7	5·7
1. 17 0	4·2	4·8

1844. Day, hour and minute.	In air. Height in feet. 8.	10.	12.
Feb. h m	°	°	°
21. 11 0	0·0	0·1	0·2
22. 6 45	1·2	1·3	1·3
22. 9 0	0·8	0·8	0·1
22. 11 30	2·4	2·1	2·4
22. 12 30	0·8	0·8	0·8
22. 13 0	1·3	1·3	1·6
22. 14 30	0·9	0·8	1·0
22. 16 0	0·6	0·8	0·7
22. 17 30	0·2	−0·1	0·1
22. 18 45	−0·1	0·0	0·0

Table XLVIII. Abstract of Special Simultaneous Observations.

Situation of the thermometer.	Excesss of air-temperature above the reading of thermometer.	Number of simultaneous observations.	Remarks.
One inch above grass	9·0	9	The observations at the height of two inches were made on one night only.
Two inches above grass	5·0		
On white unwrought cotton wool on grass	9·4	16	It would seem that the amount of radiation from the raised board was less than that from the same substance on the ground.
On white unwrought cotton wool on the raised board	8·8		
On flax on grass	9·0	11	The amount of radiation from flax on the raised board decidedly the smaller.
On flax on the raised board	6·9		
On sand on the ground	5·6	25	The sand on the board was a small quantity, whilst that on the ground was a large quantity; the amount of radiation from sand on the raised board was the smaller.
On sand on the raised board	5·1		
On zinc thin	5·1	31	From these it would appear that the thickness of a metal has no influence on the amount of its radiating power.
On zinc moderately thick	5·0		
On zinc thick	5·1		
On firestone	3·3	31	The circumstance of Purbeck stone having a result larger than the other two is probably accidental, as on many nights its results were less than those of the other stones.
On Purbeck stone	3·7		
On Portland stone	3·3		
On lead on grass	6·5	12	The increase in the readings of a thermometer placed on a metal, raised in the air, is decided.
On lead six inches above grass placed horizontally	4·5		
In air at eight feet high	0·8	10	The results are identical, and therefore in Table XLV. such should be the case in the mean. The observations at ten feet were made on one night only.
In air at ten feet high	0·8		
In air at twelve feet high	0·8		
On lead one foot high	5·5	2	The increase in the readings of a thermometer with height is indicated.
On lead three feet high	5·3		

The remarks in the last column indicate the results of the experiments; those relating to the different readings of a thermometer when placed on the same substance on the raised board and on the grass, seem to exhibit in a decided manner that more heat had passed from the board to the surface of the substance than had passed from the grass to it, causing the reading of the thermometers to be increased; the amount of this increase appears to be about half a degree, and this amount should be applied subtractively to all readings on substances placed on the raised board, or additively to all differences between their readings and those in air at the height of four feet.

In Table XLV. the results from all substances in powder were deduced from observations on the raised board, and from this investigation all these results should be increased by half a degree; in the same table the results from blue and yellow jeweller's wool and from raw silk exhibit a greater degree of cold on the raised board than when they were on grass; but these substances were never observed simultaneously in both positions, and they were only placed on the raised board on the finest and calmest nights, and in this latter position they require the same correction to be applied to them as it is necessary to apply to the other substances, whose results are deduced from observations taken on the raised board alone.

Table XLIX.

Observations connected with Snow.

1844. Day, hour and minute.	Reading of thermometer in air at four feet high protected from radiation.	Excess of the reading of a thermometer placed in air at the height of four feet and protected from the effects of radiation, above that of a thermometer fully exposed to the sky, and placed: On long grass clear of snow.	On long grass covered by snow.	On short grass clear of snow.	On short grass covered by snow.	On surface of ground under short grass clear of snow.	On surface of ground under long grass clear of snow.	One inch below surface of ground under grass covered by snow.	On grass cleared of snow covered by glass free of snow one inch high.	On grass clear of snow covered by white tin one inch high.	On snow.	On raw wool on snow.	On flax on snow.	In focus of metallic reflector cleared of snow.	Nine inches above wood cleared of snow.	One foot above snow.	Two feet above snow.	On lead six inches high cleared of snow.	On copper on snow.	On zinc on snow.	On zinc on grass cleared of snow.	On Purbeck stone cleared of snow.	On glass on snow.	On glass one inch high cleared of snow.	On white tin on snow.	On black tin on snow.	Clouds. Modification.	Clouds. Amount 0–10.	Clouds. High or low.	Wind. Direction.	Wind. Strength 0–6.	Haze, fog, mist, or vapour.	Remarks.
Feb. 4. 15 0	31·6	7·6	...	5·4	...	...	...	...	...	...	...	9·8	9·8	9·3	...	1·8	1·9	...	...	...	...	...	...	...	4·8	9·0	Clear	...	...	...	...	H.	Snow to the depth of three inches was on the ground.
4. 19 0	29·7	6·5	...	5·1	...	...	...	...	...	...	...	8·1	8·0	8·7	...	2·3	2·7	...	...	...	...	...	...	...	3·5	7·5	Cirro-stratus	9	...	c.			
5. 5 0	31·0	8·0	−2·0	6·4	−2·7	1·8	...	−5·5	...	...	5·0	7·0	6·5	9·9	...	1·7	1·7	...	...	5·2	...	...	6·8	7·8	7·8	9·7	Clear	...	...	...	...	H.	
5. 7 25	29·0	9·2	−3·0	9·6	−2·8	−1·8	...	−5·3	4·2	...	6·8	8·2	9·2	10·9	...	2·0	2·0	6·2	...	6·0	...	...	10·4	5·6	7·8	8·8	Flying clouds	3	...	...	...	H.	
6. 11 0	33·1	5·1	1·1	5·9	2·6	2·6	0·1	−1·9	1·6	3·6	2·6	7·6	8·0	7·6	2·1	0·9	0·9	...	5·1	3 9	3·6	2·6	5·6	4·0	5·6	6·1	Cirro-stratus	9½	H.	c.			

By taking the mean of the numbers in each column, the following results are obtained:—

Table L. Experiments connected with Snow.

Situation of the thermometer, its bulb, or the substance under which it was placed being fully exposed to the sky.	Mean excess of the reading of the thermometer in air at the height of four feet, protected from radiation.	Number of comparisons.
On long grass clear of snow	7·3	5
On long grass covered by snow	−1·3	3
On short grass clear of snow	5·5	5
On short grass covered by snow	−1·0	3
On surface of ground under short grass clear of snow	0·9	3
On surface of ground under long grass clear of snow	0·1	1
One inch below surface of ground under long grass covered by snow	−4·2	3
On grass cleared of snow covered by glass free from snow one inch high	2·9	2
On grass clear of snow covered by white tin one inch high	3·6	1
On snow	4·8	3
On raw wool on snow	8·2	5
On flax on snow	8·3	5
In focus of metallic reflector cleared of snow	9·3	5
Nine inches above wood cleared of snow	2·1	1
One foot above snow	1·7	5
Two feet above snow	1·8	5
On lead six inches high cleared of snow	6·2	1
On copper on snow	5·1	1
On zinc on snow	5·0	3
On zinc on grass cleared of snow	3·6	1
On Purbeck stone cleared of snow	2·6	1
On glass on snow	7·6	3
On glass one inch high cleared of snow	5·8	3
On white tin on snow	5·9	5
On blackened tin on snow	8·2	5

At the time of two of these five sets of observations the sky was nearly covered with cloud; at one other time one-third part of the sky was clouded over and much haze prevailed; and at the remaining times the sky was not bright, and there was a perceptible haze. The mean temperature of the air was 30°·9; the mean reading of the thermometer on long grass clear of snow was 1°·8 less than that on short grass, also clear of snow; and the readings were nearly alike when the two were covered with snow. The effects of snow on long grass was to cause the thermometers on the grass to read higher by 8°·6, but this difference does not fully represent the non-conducting power of snow, as the temperature of the air varied very little whilst these experiments were being made; had the temperature of the air fallen or risen much whilst the snow had been on the ground, the temperature of the earth under the snow would have continued nearly at the same reading, and in that case the effects of the snow would have been much more decided. The next favourable opportunity I had of repeating my experiments on snow was 1845, February 12*. Snow had previously fallen to the depth of three inches, and during the night, which was cloudless, the reading of a thermometer which was placed on long grass was −6°, whilst that covered by snow was 28°·0; the effect of the snow was therefore to keep the grass warmer by 34°, and therefore vegetation was kept warmer by this amount than it would have been had there been no snow. The reading of a thermometer placed on the snow was −12°, and the difference between the readings of two thermometers, the one placed on snow and the other under the snow, was 40°. After this time the reading on grass clear of snow rose to 24°, and that on snow increased to 15°, without causing any variation in the reading of that under the snow, which still read 28°. Snow would therefore appear to be a very perfect non-conductor of heat. The lowest reading of a thermometer on flax was $-12°\frac{1}{2}$; that on short grass was 5°, being 11° higher than that on long grass; this heat (see remarks following Table XLV.) represents the difference between the quantities of heat conducted from beneath the surface to long and to short grass, and it would represent also the greater quantity of heat lost from the earth as covered by short grass than that by long grass; and if to this 11° we add the heat conducted beneath the surface to long grass, which did not differ much from 6°, it would appear that heat to the amount of 17° was conducted to the short grass from the earth beneath it. Snow being so perfect a non-conductor of heat, evidently prevents to a high degree the loss of heat by radiation from bodies covered by it; and it also prevents the loss of heat from such bodies by conduction, at times when the temperature of the air is lower than they are. Raw wool, flax, straw, and other bodies which are bad conductors of heat, act in a similar way and prevent the injurious effects of cold to bodies covered by them, to which injurious effects vegetation is liable in this climate in every month of the year, as it is liable to a temperature at night below the freezing-point of water in every month.

* For these observations, see the Greenwich Magnetical and Meteorological volume for the year 1845, pages 256 and 257.

Section II.—*Results of Observations made by Self-registering Minimum Thermometers.*

These observations extend over a period of time of nearly four years' duration, viz. from 1841 February to the end of 1844*.

The observations consist of the daily reading of a self-registering minimum thermometer with its bulb placed at the height of four feet above the ground, protected from the effects of radiation and rain, but in other respects freely exposed to the air, and the daily reading of a similar thermometer placed with its bulb in the focus of a metallic parabolic reflector, and fully exposed to the sky.

The kind and average amount of cloud by which the sky was covered were also noted every night. The average direction and strength of the wind were also determined every night.

From 1843 April and extending to the end of 1844, several similar thermometers were placed on or near different substances, and their readings were taken daily.

The first process in the reduction of the observations was the taking the difference between the reading of the thermometer in air and the simultaneous reading of every other thermometer.

The second process was the forming all those differences into groups, according to the variable state of the sky, depending only on the kind and amount of cloud, but independently of its height. The result of this investigation was found to be that the amount of the difference varied with every variation of the amount of the clouds.

The third process was the forming groups of the differences derived from the same cloudy state of the sky, but with the clouds at different distances from the earth. The result was found to be that the amount of the numbers was different according to the variable distance that the clouds were from the earth.

The fourth step was the forming groups of numbers derived from observations on different nights with the same state of the sky, but with the wind blowing from different quarters, independently of its strength. The result of this investigation was that no certain difference existed depending on the quarter from whence the wind blew.

The fifth step was the forming groups of the numbers found from observations on different nights with the same state of the sky, on calm nights, and on nights when the wind has been blowing strongly, at times amounting to a gale, independently of its direction. The result was found to be that no certain difference existed, depending on the strength or velocity of the wind, showing clearly that on every windy night a portion of time had been calm of sufficient duration for the instruments to register the loss of heat by radiation, and to the same amount as would have been shown had the air been in a calm state during the night.

Having thus explained the manner in which the preliminary steps in the reduction

* The observations are placed in the Archives of the Royal Society.

of the observations have been made, I shall proceed with the explanation of the process for the formation of the following tables.

As the variations of the excess of the readings of the thermometer in the air, above those otherwise placed, were thus found to depend on the variations in the amount and in the height of the clouds only, the next operation was dividing the results of each month's observations into five groups as follow.

The first group was formed from the observations taken on nights which were cloudy throughout.

The second group from those taken on nights which were principally cloudy, but the clouds frequently broken.

The third group from those taken on nights which were half-clear and half-cloudy.

The fourth group from those taken on nights which were principally cloudless, but yet clouds were frequent.

The fifth group from those taken on cloudless nights, and in this way Tables LI. to XCIX. were formed; the successive results in each class being arranged according to their dates of occurrence.

The examination of the columns in each month as to the distribution of the numbers, gives a good knowledge of the distribution of the clouds at night during the month.

Excess of the reading of a self-registering minimum thermometer placed in air at the height of four feet, protected from the effects of radiation, above that of a similar thermometer placed in the focus of a metallic parabolic reflector fully exposed to the sky on every night, arranged according to the state of the sky with respect to the quantity of clouds.

Number of the Tables.	1841. Month.	General state of the sky during the night previous to reading the instruments.									
		Cloudy.		Broken clouds.		Half-cloudy.		Principally clear.		Cloudless.	
		Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.
		d h	°	d h	°	d h	°	d h	°	d h	°
LI.	Feb.	4 21	4·8	24 21	3·8	17 21	....	....	....	19 21	5·8
		5 21	3·0	28 21	4·1	18 21	3·2				
		6 21	0·8								
		7 21	2·8								
		8 21	2·3								
		9 21	2·8								
		10 21	5·7								
		11 21	1·9								
		12 21	1·7								
		13 21	0·8								
		14 21	−0·6								
		15 21	1·2								
		16 21	2·9								
		20 21	2·3								
		21 21	2·1								
		22 21	1·1								
		23 21	1·5								
		25 21	2·4								
		27 21	1·3								
LII.	March	13 21	−0·5	1 21	1·8	7 21	5·8	2 21	5·2	6 21	9·4
				4 21	2·1	9 21	5·3	3 21	4·9	8 21	7·2
		30 21	2·0	20 21	3·5	14 21	4·2	5 21	5·1	10 21	4·7
		31 21	0·2	23 21	4·0	22 21	4·3	17 21	9·7	11 21	7·0
				28 21	4·4	26 21	5·9	18 21	5·4	12 21	5·3
										15 21	8·8
										16 21	13·4
										19 21	6·7
										24 21	7·3
										25 21	6·9
										27 21	5·7
										29 21	2·7
LIII.	April	10 21	2·1	1 21	3·5	5 21	4·5	14 21	7·2	2 21	8·6
		13 21	2·8	6 21	2·7	9 21	4·7	21 21	3·5	3 21	7·8
		22 21	0·7	7 21	1·9	12 21	6·6	23 21	6·5	4 21	11·5
		24 21	2·1	11 21	1·3	19 21	7·9	26 21	5·6	8 21	11·8
				18 21	(−2·4)	20 21	4·7	28 21	5·6	15 21	7·6
				25 21	6·3	27 21	5·4	29 21	6·2	16 21	10·2
										30 21	8·1

TABLE (Continued).

Number of the Tables.	1841. Month.	General state of the sky during the night previous to reading the instruments.									
		Cloudy.		Broken clouds.		Half-cloudy.		Principally clear.		Cloudless.	
		Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.
		d h	°	d h	°	d h	°	d h	°	d h	°
LIV.	May	2 21	0·2	5 21	3·9	10 21	5·3	24 21	6·6	1 21	7·7
		3 21	0·1	6 21	3·2	16 21	5·5	28 21	5·4	12 21	8·5
		4 21	−0·1	7 21	−0·4	17 21	5·4	30 21	7·5	13 21	7·8
		21 21	0·2	8 21	2·5	19 21	3·7	31 21	8·4	14 21	7·5
		25 21	0·6	9 21	1·1	22 21	5·5	....	....	15 21	8·0
		29 21	1·5	11 21	3·5	23 21	5·4	....	....	20 21	5·9
				18 21	0·5	....	....	....	....	26 21	5·3
				27 21	2·1						
LV.	June	5 11	−0·7	2 21	2·3	4 21	7·9	1 21	5·9	3 21	9·2
		7 21	−1·1	6 21	5·6	16 21	5·1	10 21	7·8	9 21	9·4
		8 21	0·8	14 21	3·7	19 21	4·9	13 21	6·7	12 21	11·2
		11 21	−0·4	....	....	21 21	4·9	15 21	8·3	20 21	3·5
		18 21	1·4	28 21	1·9	22 21	4·6	17 21	9·4	26 21	(−0·3)
		24 21	....	30 21	3·5	25 21	(−0·3)	23 21	6·2		
		27 21	0·6	....	....	29 21	4·5				
LVI.	July	1 21	−0·3	4 21	3·6	12 21	5·3	4 21	7·0	3 21	10·9
		2 21	1·4	11 21	2·8	18 21	5·2	8 21	2·1	9 21	3·8
		3 21	1·3	14 21	1·9	24 21	4·6	13 21	4·9	16 21	8·0
		7 21	−0·3	19 21	3·1	28 21	4·1	....	....	17 21	6·8
		10 21	0·3	20 21	3·6	29 21	4·7				
		15 21	1·5	21 21	2·9	30 21	4·2				
		23 21	1·7	22 21	2·3						
				25 21	2·1						
				26 21	3·4						
				27 21	3·7						
				31 21	2·2						
LVII.	August	1 21	0·1	4 21	5·0	6 21	7·9	9 21	3·3	20 21	7·0
		2 21	−0·6	5 21	3·5	11 21	4·5	15 21	6·5	24 21	10·5
		3 21	−1·3	7 21	3·8	12 21	6·5	18 21	6·4	28 21	8·2
		10 21	0·5	8 21	3·0	14 21	5·5	19 21	5·2	29 21	14·0
		17 21	1·8	13 21	2·5	23 21	5·1	21 21	6·2		
		22 21	0·3	16 21	3·9	30 21	5·0	27 21	6·0		
		25 21	1·6	26 21	3·2						
		31 21	2·2								
LVIII.	Sept.	4 21	2·9	27 21	4·0	3 21	8·4	1 21	6·7	6 21	4·5
		20 21	2·3	29 21	4·5	5 21	5·6	2 21	6·1	10 21	10·5
						7 21	5·3	13 21	3·6	12 21	11·7
						8 21	4·5	16 21	5·8	15 21	7·1
						9 21	4·8	....	....	17 21	7·1
						11 21	4·1	....	....	18 21	7·9
						14 21	5·5				
						19 21	4·1				
						21 21	3·3				
						22 21	4·3				
						23 21	4·7				
						24 21	5·2				
						25 21	4·3				
						26 21	2·7				
						28 21	4·0				
						30 21	4·1				

Table (Continued).

Number of the Tables.	1841. Month.	General state of the sky during the night previous to reading the instruments.									
		Cloudy.		Broken clouds.		Half-cloudy.		Principally clear.		Cloudless.	
		Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.
		d h	°	d h	°	d h	°	d h	°	d h	°
LIX.	Oct.	27 21	0·6	1 21	3·6	6 21	2·4	15 21	7·0	16 21	8·2
		28 21	2·1	2 21	2·5	7 21	2·4	....	....	17 21	12·9
		29 21	2·7	3 21	1·0	8 21	3·9	....	....	19 21	6·7
		30 21	0·0	4 21	3·1	9 21	4·6	....	....	20 21	7·9
		31 21	2·1	5 21	3·1	12 21	4·8	....	....	21 21	7·9
				10 21	3·8	14 21	4·1				
				11 21	3·2	18 21	4·5				
				13 21	3·4	22 21	5·9				
				26 21	2·1	23 21	4·7				
						24 21	4·2				
						25 21	3·5				
LX.	Nov.	1 21	2·4	3 21	2·2	2 21	8·9	22 21	7·0	6 21	9·0
		21 21	2·0	4 21	−1·1	5 21	4·3	23 21	6·7	8 21	8·8
		27 21	2·7	7 21	4·1	10 21	5·7	26 21	7·9	11 21	9·4
				9 21	3·2	24 21	5·6	....	....	12 21	5·4
				13 21	0·9	25 21	5·7	....	....	14 21	8·2
				15 21	1·1	30 21	6·1	....	....	16 21	10·0
				20 21	2·4	....	....	....	....	17 21	9·2
				28 21	4·9	....	....	....	....	18 21	8·6
				29 21	5·0	....	....	....	....	19 21	3·6
LXI.	Dec.	19 21	4·5	2 21	5·3	1 21	5·1	3 21	6·9	6 21	8·2
		28 21	4·3	7 21	3·9	4 21	5·0	8 21	7·1	10 21	7·8
		29 21	4·2	12 21	4·0	5 21	5·9	16 21	8·0	14 21	8·9
				24 21	1·0	9 21	6·1	21 21	6·6	17 21	8·9
				25 21	3·7	11 21	5·9	23 21	6·7	18 21	11·8
				27 21	4·4	13 21	4·4	....	....	20 21	8·5
						15 21	5·8	....	....	26 21	6·5
						22 21	8·2				
						30 21	5·6				
LXII.	1842. Jan.	8 21	2·1	0 21	5·0	1 21	7·6	6 21	5·5	7 21	6·0
		9 21	2·5	2 21	2·7	3 21	8·7	12 21	7·8	14 21	9·1
		10 21	4·5	....	....	4 21	4·7	15 21	7·0	23 21	10·7
		11 21	1·8	....	....	5 21	4·5	16 21	4·9	24 21	10·7
		13 21	2·4	....	....	17 21	3·6	22 21	6·4	25 21	8·1
		18 21	2·0	....	....	28 21	6·6	26 21	5·5	27 21	12·0
		19 21	2·2	....	....	30 21	5·3	31 21	6·1		
		20 21	0·6								
		21 21	3·8								
		29 21	1·5								
LXIII.	Feb.	7 21	1·6	2 21	3·7	3 21	4·3	1 21	7·3	13 21	6·0
		11 21	1·0	4 21	3·3	5 21	5·0	6 21	5·7	15 21	8·9
		16 21	2·8	8 21	1·2	12 21	7·2	14 21	5·4	17 21	8·4
		23 21	1·4	9 21	2·5	20 21	6·4	18 21	5·0	25 21	9·3
		24 21	2·3	10 21	3·8	21 21	5·4	19 21	5·5		
				22 21	3·9	27 21	5·4				
						28 21	5·6				

TABLE (Continued).

Number of the Tables.	1842. Month.	General state of the sky during the night previous to reading the instruments.									
		Cloudy.		Broken clouds.		Half-cloudy.		Principally clear.		Cloudless.	
		Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.
		d h	°	d h	°	d h	°	d h	°	d h	°
LXIV.	March	2 21	3·2	19 21	1·6	3 21	4·1	18 21	7·4	1 21	7·4
		7 21	1·4	....	....	5 21	4·9	22 21	5·6	4 21	4·1
		9 21	2·5	....	....	8 21	4·3	23 21	6·7	6 21	6·4
		14 21	0·5	....	....	11 21	4·1	....	....	10 21	8·2
		15 21	1·3	....	....	16 21	6·4	....	....	12 21	6·8
		24 21	2·5	....	....	17 21	4·5	....	....	13 21	6·5
		29 21	2·5	....	....	20 21	3·8	....	....	25 21	6·3
		31 21	1·6	....	....	21 21	3·6	....	....	26 21	6·3
						27 21	3·3				
						28 21	3·2				
						30 21	4·2				
LXV.	April	12 21	2·2	11 21	3·9	1 21	4·9	3 21	7·9	5 21	7·5
		13 21	0·9	....	....	2 21	4·9	4 21	7·7	8 21	4·4
		14 21	2·8	....	....	7 21	4·8	6 21	3·8	17 21	8·1
		18 21	0·7	....	....	9 21	4·6	15 21	3·7	19 21	5·6
		21 21	0·6	....	....	10 21	4·3	16 21	4·3	25 21	6·6
						22 21	4·6	20 21	2·5	26 21	7·0
						23 21	6·9	24 21	5·9	27 21	9·3
						29 21	3·7	....	....	28 21	7·6
										30 21	7·7
LXVI.	May	3 21	2·3	5 21	5·7	7 21	5·9	4 21	6·5	1 21	8·1
		11 21	1·7	6 21	1·6	14 21	4·4	8 21	6·7	2 21	7·1
		27 21	1·7	12 21	6·1	15 21	5·7	10 21	7·4	9 21	9·0
				20 21	5·3	18 21	7·7	16 21	6·9	13 21	7·6
				21 21	6·4	25 21	6·9	17 21	5·2	23 21	9·5
								19 21	6·6	28 21	8·4
								22 21	8·2	29 21	9·2
								24 21	6·8	31 21	8·7
								26 21	6·2		
								30 21	7·2		
LXVII.	June	25 21	2·4	24 21	5·8	14 21	5·2	1 21	8·3	2 21	9·7
		30 21	−0·1	....	....	16 21	6·5	4 21	6·5	3 21	8·9
						17 21	6·2	13 21	5·8	5 21	9·1
						18 21	6·2	15 21	6·7	6 21	15·6
						19 21	5·5	21 21	5·3	7 21	16·0
						20 21	5·8	27 21	5·7	8 21	7·9
						23 21	4·9	28 21	6·0	9 21	7·3
								29 21	7·0	10 21	6·4
										11 21	6·9
										12 21	7·2
										22 21	12·8
										26 21	14·2

Table (Continued).

Number of the Tables.	1842. Month.	General state of the sky during the night previous to reading the instruments.									
		Cloudy.		Broken clouds.		Half-cloudy.		Principally clear.		Cloudless.	
		Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.
		d h	°	d h	°	d h	°	d h	°	d h	°
LXVIII.	July	7 21	5·9	18 21	4·4	1 21	5·2	2 21	5·7	13 21	10·2
		22 21	2·6	19 21	4·6	3 21	4·9	5 21	7·2	14 21	10·4
		27 21	2·6	28 21	2·3	4 21	2·5	6 21	8·0	15 21	7·9
						8 21	4·8	9 21	5·9	23 21	8·1
						11 21	5·1	10 21	6·1	24 21	9·0
						20 21	6·9	12 21	5·8	25 21	6·6
								16 21	5·6		
								17 21	8·1		
								21 21	6·7		
								26 21	4·4		
								29 21	6·1		
								30 21	7·1		
								31 21	7·3		
LXIX.	August	10 21	1·5	19 21	3·6	4 21	6·0	2 21	5·7	1 21	7·0
		24 21	2·8	....	....	5 21	4·5	12 21	5·5	3 21	3·4
		25 21	4·5	....	....	6 21	4·4	13 21	5·5	7 21	6·6
						18 21	7·8	26 21	6·2	8 21	7·4
						20 21	4·4	....	....	9 21	8·3
						22 21	5·5	....	....	11 21	6·7
						23 21	6·8	....	....	14 21	7·3
						27 21	4·3	....	....	15 21	7·2
						29 21	0·4	....	....	16 21	4·1
						30 21	5·2	....	....	17 21	3·3
						31 21	6·0	....	....	21 21	7·3
										28 21	−0·6
LXX.	Sept.	1 21	1·1	7 21	3·6	4 21	6·3	3 21	1·4	2 21	2·5
		11 21	3·2	8 21	4·8	9 21	5·2	5 21	8·0	14 21	6·4
		17 21	1·7	25 21	1·7	13 21	5·7	6 21	6·5	15 21	4·6
		23 21	2·3	26 21	0·5	16 21	1·0	10 21	6·1		
		24 21	1·7	....	....	18 21	3·9	12 21	6·6		
						22 21	0·1				
						27 21	5·7	19 21	5·0		
						28 21	4·4	20 21	6·1		
						30 21	5·7	21 21	4·6		
								29 21	6·8		
LXXI.	Oct.	9 21	3·4	12 21	5·0	8 21	4·9	1 21	4·0	2 21	7·0
		14 21	3·0	17 21	4·1	11 21	5·8	5 21	8·0	3 21	8·7
		15 21	3·2	27 21	4·8	13 21	9·7	6 21	8·8	4 21	10·1
		16 21	2·5	....	....	26 21	4·4	7 21	7·1	19 21	9·2
		18 21	1·7	....	....	....	....	10 21	7·8	20 21	11·1
		22 21	2·8	....	....	....	....	23 21	3·9	21 21	11·4
		30 21	2·5	....	....	....	....	24 21	5·6	28 21	5·2
								25 21	7·5	29 21	14·2
										31 21	5·2

Table (Continued).

Number of the Tables.	1842. Month.	General state of the sky during the night previous to reading the instruments.									
		Cloudy.		Broken clouds.		Half-cloudy.		Principally clear.		Cloudless.	
		Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Day and hour of reading.	Excess of the reading of the thermometer in air above that placed in the focus of the reflector.
		d h	°	d h	°	d h	°	d h	°	d h	°
LXXII.	Nov.	7 21	(9·3)	13 21	4·4	11 21	4·2	1 21	8·9	2 21	11·9
		8 21	4·5	23 21	4·5	21 21	8·7	3 21	9·1	16 21	8·0
		9 21	(9·0)	....	....	24 21	4·7	4 21	8·0	18 21	11·9
		10 21	3·1	....	....	28 21	5·5	5 21	7·3		
		14 21	4·5	....	....	29 21	6·7	6 21	9·3		
		15 21	1·0	....	....	....	....	12 21	4·9		
		17 21	(9·4)	....	....	....	....	22 21	5·0		
		19 21	2·0	....	....	....	....	25 21	7·0		
		20 21	8·7	....	....	....	....	26 21	8·4		
								27 21	6·6		
								30 21	8·2		
LXXIII.	Dec.	1 21	3·3	9 21	8·0	4 21	5·5	13 21	7·1	2 21	9·4
		3 21	1·8	....	....	16 21	4·8	15 21	8·2	5 21	4·8
		6 21	2·0	....	....	22 21	5·1	18 21	2·8	14 21	7·7
		7 21	2·3	....	....	....	....	23 21	4·6	17 21	5·7
		8 21	5·1	....	....	....	....	28 21	5·3	24 21	6·5
		10 21	3·0	....	....	....	....	31 21	9·4	27 21	5·2
		11 21	1·2								
		12 21	5·7								
		19 21	3·7								
		20 21	1·8								
		21 21	2·3								
		25 21	3·0								
		26 21	1·7								
		29 21	1·8								
		30 21	3·1								
LXXIV.	1843. Jan.	....	....	3 21	(8·2)	9 21	8·4	12 21	6·9	1 21	10·9
		11 21	4·8	6 21	3·9	13 21	5·5	15 21	9·1	2 21	11·7
		19 21	2·0	7 21	1·8	14 21	5·9	30 21	5·8	4 21	7·7
		22 21	(6·2)	17 21	3·8	24 21	6·3	....	....	5 21	9·5
		23 21	(9·9)	18 21	4·5	25 21	4·4	....	....	8 21	10·0
		26 21	3·6	20 21	5·4	28 21	5·7	....	....	10 21	6·7
		27 21	3·8	29 21	5·0	....	....	....	....	16 21	8·5
		31 21	2·8		....	....	....	....	....	21 21	11·1
LXXV.	Feb.	7 21	2·8	3 21	(0·3)	2 21	6·4	1 21	5·6	10 21	9·1
		8 21	3·0	4 21	3·5	6 21	7·7	5 21	8·9	12 21	14·1
		9 21	4·7	15 21	10·8	17 21	9·7	....	....	13 21	11·4
		11 21	3·1	....	....	21 21	12·9	....	....	16 21	13·0
		14 21	(13·3)	....	....	23 21	5·4	....	....	22 21	7·3
		18 21	3·0								
		19 21	0·6								
		20 21	3·9								
		24 21	0·8								
		25 21	(8·5)								
		26 21	3·9								
		27 21	3·0								
		28 21	7·8								

Table (Continued).

Number of the Tables.	1843. Month.	General state of the sky during the night previous to reading the instruments. Cloudy. Day and hour of reading.	Cloudy. Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Broken clouds. Day and hour of reading.	Broken clouds. Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Half-cloudy. Day and hour of reading.	Half-cloudy. Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Principally clear. Day and hour of reading.	Principally clear. Excess of the reading of the thermometer in air above that placed in the focus of the reflector.	Cloudless. Day and hour of reading.	Cloudless. Excess of the reading of the thermometer in air above that placed in the focus of the reflector.
		d h	°	d h	°	d h	°	d h	°	d h	°
LXXVI.	March	10 21	11·4	30 21	10·5	21 21	7·1	12 21	7·5	1 21	12·0
		11 21	4·3	....	....	22 21	7·9	16 21	8·7	2 21	17·6
		13 21	3·8	....	....	26 21	6·8	20 21	8·0	3 21	12·1
		14 21	2·0	....	....	....	....	23 21	8·7	4 21	11·2
		15 21	2·5	....	....	....	....	24 21	8·9	5 21	10·5
		27 21	5·7	....	....	....	....	....	....	6 21	14·2
		31 21	7·1	....	....	....	....	....	....	7 21	11·5
										8 21	10·5
										9 21	12·7
										17 21	11·2
										18 21	10·0
										19 21	6·0
										25 21	9·2
										28 21	12·3
										29 21	12·6
LXXVII.	April	1 21	3·1	2 21	2·8	3 21	6·4	8 21	5·2	4 21	8·3
		6 21	1·8	....	....	5 21	3·1	10 21	6·8	9 21	9·4
		14 21	3·8	....	....	7 21	4·2	16 21	6·6	11 21	9·4
		15 21	2·4	....	....	12 21	6·9	19 21	9·5	13 21	9·2
						20 21	5·3	21 21	9·0	17 21	10·3
						25 21	4·5	29 21	7·3	18 21	9·0
						26 21	5·8	....	....	22 21	13·0
						28 21	6·6	....	....	23 21	12·0
										24 21	12·4
										27 21	9·4
										30 21	9·1

Table LXXVIII.

General state of the sky during the night previous to reading the instruments.	1843. April. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air at the height of 4 feet, and protected from the effects of radiation above that placed On long grass exposed to $\frac{3}{4}$ths of sky.	On short grass.	On garden mould.	One inch high.	Three inches high.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°
Cloudless.	22 21	37·0	....	11·2	4·0	....	....	13·0
	23 21	36·0	....	6·8	4·3	....	....	12·0
	30 21	48·6	8·1	7·6	8·6	8·6	7·9	9·1
Principally cloudless.	19 21	42·3	....	9·3	0·3	....	....	9·3
	24 21	34·2	....	12·7	4·4	....	....	12·4
Half-cloudy.	20 21	42·3	....	3·3	0·3	....	....	3·3
	21 21	48·0	....	9·0	5·2	....	....	9·0
	26 21	38·2	4·7	6·2	4·4	4·7	4·2	6·7
Principally cloudy.	27 21	37·4	11·7	9·7	1·1	8·4	5·1	9·4
Cloudy.	28 21	41·8	4·0	5·6	2·3	4·8	3·0	6·6
	29 21	46·0	7·3	8·1	6·2	6·0	7·3	7·3

Table LXXIX.

General state of the sky during the night previous to reading the instruments.	1843. May. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed					
			On long grass exposed to $\frac{3}{4}$ths of sky.	On short grass.	On garden mould.	One inch high.	Three inches high.	In focus of metallic reflector.
	d h							
Cloudless.	1 21	46°·3	5°·0	8°·4	3°·0	9°·1	5°·2	10°·5
	10 21	39·9	6·9	11·9	4·9	13·7	10·1	10·4
Principally clear.	2 21	40·7	2·9	7·0	1·7	6·8	4·1	7·7
	6 21	35·4	1·4	1·2	1·4	4·8	3·6	6·4
	7 21	37·3	4·0	1·4	3·7	9·9	8·0	9·3
	11 21	40·3	4·4	10·4	4·4	10·8	9·1	8·8
	14 21	54·5	7·0	7·8	5·9	7·5	7·8	6·9
	25 21	47·3	3·3	3·2	3·0	6·8	6·8	5·8
	28 21	45·2	8·7	4·0	3·0	6·5	7·7	9·0
Half-cloudy.	4 21	47·1	7·3	10·9	3·5	10·1	8·7	4·8
	21 21	44·4	1·9	1·4	2·4	5·4	6·6	5·1
	22 21	48·1	2·7	5·1	3·0	6·4	6·2	5·2
	24 21	48·5	4·5	9·0	8·0	7·3	8·8	5·3
Principally cloudy.	9 21	42·8	3·8	4·3	2·2	8·8	6·9	7·3
	19 21	47·5	7·0	4·3	7·3	......	7·3	7·7
	26 21	47·3	3·3	4·3	2·3	4·3	3·3	5·8
	27 21	44·8	3·3	4·6	2·2	4·6	5·0	3·0
	29 21	38·5	3·5	3·0	3·3	9·5	9·0	9·2
	31 21	55·3	2·1	1·1	1·5	1·9	2·1	1·5
Cloudy.	3 21	37·3	−2·7	1·3	−4·2	1·3	0·2	−0·2
	5 21	50·2	2·4	0·9	1·4	2·7	2·7	2·0
	8 21	38·0	1·0	−1·0	0·0	1·5	1·2	−4·0
	12 21	51·3	7·5	5·5	6·0	3·3	7·6	2·1
	13 21	45·0	3·0	4·5	−0·5	4·5	3·5	3·8
	15 21	49·4	5·4	2·1	2·4	3·4	3·2	3·7
	16 21	49·3	9·3	6·3	7·3	......	10·3	5·0
	17 21	44·0	2·0	0·0	1·8	4·0	4·0	4·5
	18 21	44·8	1·7	−0·9	0·7	1·5	2·9	3·8
	20 21	50·3	7·0	7·0	4·1	6·7	9·3	8·6
	23 21	52·6	4·6	6·1	5·8	8·9	9·0	2·8
	30 21	50·3	3·3	1·9	2·2	3·0	3·1	2·8

Table LXXX.

General state of the sky during the night previous to reading the instruments.	1843. June. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed On long grass exposed to $\frac{3}{4}$ths of sky.	On short grass.	On garden mould.	One inch high.	Three inches high.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°
Cloudless.	3 21	47·1	4·3	8·1	3·8	8·6	8·9	8·5
	16 21	48·4	4·9	8·5	3·4	11·3	10·5	10·4
	21 21	53·0	9·0	11·0	8·0	8·0	12·0	13·0
	22 21	44·8	4·8	11·1	4·6	10·8	11·0	10·2
	23 21	50·4	7·2	6·6	5·9	12·9	12·7	12·4
	25 21	45·3	4·6	11·1	4·2	11·6	11·3	8·0
	28 21	43·5	10·2	11·7	7·5	10·5	12·5	9·3
Principally clear.	2 21	51·3	2·3	2·3	1·9	3·0	2·5	3·3
	4 21	42·9	1·5	4·1	1·7	5·4	5·4	5·6
	5 21	43·4	3·6	8·1	3·1	9·9	8·7	7·4
	15 21	51·6	4·4	5·1	3·0	9·6	10·4	11·2
	18 21	50·5	2·5	5·5	1·4	7·5	7·4	5·4
	20 21	44·1	4·1	8·5	3·6	10·8	9·8	10·9
	29 21	52·1	6·3	7·9	5·5	12·0	11·3	9·5
Half-cloudy.	26 21	47·0	3·8	7·3	4·0	10·8	10·6	8·0
Principally cloudy.	1 21	53·5	6·9	11·5	6·3	6·5	10·5	4·4
	6 21	45·2	2·6	3·6	2·2	6·2	5·5	6·0
	8 21	51·2	1·7	2·2	3·0	3·2	2·3	2·7
	9 21	49·1	2·8	3·5	2·9	3·6	3·1	4·3
	10 21	47·8	8·3	5·8	7·8	5·8	6·0	6·3
	11 21	48·8	4·2	5·3	3·6	5·0	6·5	6·7
	24 21	49·8	2·7	1·2	0·3	1·1	2·0	2·3
	27 21	50·2	3·4	1·9	4·2	6·0	6·9	7·2
	30 21	52·3	8·3	8·3	9·5	9·5	11·3	10·1
Cloudy.	7 21	49·9	2·1	1·1	1·2	3·9	1·8	1·4
	12 21	48·8	......	......	......	0·8	......	2·2
	13 21	51·8	1·3	−1·7	1·8	0·3	1·6	0·8
	14 21	57·0	4·5	5·9	3·6	6·8	7·1	6·4
	17 21	50·6	0·6	1·6	−1·9	3·0	2·1	2·0
	19 21	50·3	3·2	0·6	1·4	......	3·2	3·3

Table LXXXI.

General state of the sky during the night previous to reading the instruments.	1843. July. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed					
			On long grass exposed to $\frac{3}{4}$ths of sky.	On short grass.	On garden mould.	One inch high.	Three inches high.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°
Cloudless.	4 21	57·1	10·6	13·6	10·6	11·3	10·1	8·9
	9 21	51·3	4·1	11·8	......	12·8	9·8	11·3
Principally clear.	2 21	58·6	7·1	8·6	7·6	8·6	8·6	10·1
	7 21	53·4	6·6	5·4	......	6·2	5·2	6·2
	8 21	47·3	7·3	7·3	......	7·3	7·3	7·1
	16 21	58·4	4·3	4·3	......	5·4	4·8	6·3
	17 21	58·1	4·1	4·1	......	......	4·4	6·1
	19 21	46·1	6·1	6·1	......	6·1	6·1	7·1
	23 21	44·6	5·2	5·2	......	7·4	7·6	9·4
	24 21	46·1	9·6	9·6	......	9·9	9·9	10·1
	25 21	51·1	4·5	4·5	......	8·6	8·3	7·5
	29 21	53·5	4·0	4·0	......	4·0	4·0	5·9
Half-cloudy.	1 21	53·1	2·8	7·1	7·8	4·9	9·5	9·1
	3 21	59·1	2·3	3·1	1·1	3·3	2·6	4·6
	6 21	52·2	3·7	5·2	......	5·9	5·5	6·8
	14 21	55·4	3·9	10·4	......	5·4	8·6	5·4
	15 21	57·2	3·3	7·2	......	6·4	5·0	6·2
	30 21	51·9	5·4	5·4	......	7·0	6·6	7·9
	31 21	50·1	5·1	5·1	......	8·1	7·3	6·1
Principally cloudy.	5 21	55·8	3·0	1·8	......	......	3·0	3·6
	10 21	54·8	1·2	0·8	......	5·4	0·8	1·6
	21 21	55·1	4·7	4·7	......	6·8	6·4	8·1
	22 21	51·7	1·7	1·7	......	3·7	4·7	1·1
	26 21	57·3	4·8	4·8	......	5·1	4·8	6·8
	27 21	56·6	4·6	4·6	......	4·6	4·4	6·4
Cloudy.	11 21	52·8	5·8	1·2	......	......	1·8	3·8
	12 21	55·3	1·3	1·1	......	1·1	0·9	1·8
	18 21	52·3	3·8	3·8	......	9·6	5·1	7·3
	20 21	54·1	4·0	4·0	......	4·0	4·1	5·1
	28 21	53·1	0·2	0·2	......	−0·1	−0·2	1·1

Table LXXXII.

General state of the sky during the night previous to reading the instruments.	1843. August. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of the thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed				
			On long grass exposed to ¾ths of sky.	On short grass.	One inch high.	Three inches high.	In focus of metallic reflector.
	d h	°	°	°	°	°	°
Cloudless.	6 21	48·7	4·7	6·9	7·2	7·4	7·3
	10 21	47·2	6·4	12·2	13·0	13·0	9·4
	11 21	51·0	6·2	9·0	9·1	9·5	9·0
	12 21	49·7	6·7	13·7	12·9	13·3	9·9
	17 21	59·6	6·0	11·0	11·6	11·0	9·0
	18 21	59·0	5·0	11·0	11·0	11·4	10·0
Principally cloudless.	1 21	49·4	2·8	4·5	4·6	4·6	6·5
	5 21	53·6	3·6	4·6	4·6	4·4	6·4
	7 21	60·0	7·0	8·8	9·0	8·0	11·8
	9 21	57·5	6·2	6·8	6·7	6·9	5·0
	13 21	58·6	5·8	8·8	8·4	8·9	10·4
	16 21	56·8	4·8	7·5	7·3	7·4	7·6
	20 21	50·2	4·0	6·6	6·2	6·8	8·2
	21 21	51·0	3·0	6·8	6·8	7·6	9·2
	22 21	49·5	2·8	4·5	4·5	4·7	6·0
	24 21	50·0	5·6	9·7	9·4	10·0	9·8
	26 21	49·2	4·9	8·0	8·1	8·8	7·5
	31 21	56·2	5·4	8·0	8·4	8·5	8·2
Half-cloudy.	3 21	56·0	3·2	9·0	4·0	8·0	5·8
	4 21	52·6	4·5	4·5	5·0	5·0	6·6
	14 21	61·8	6·4	4·0	6·8	4·0	5·8
	15 21	61·8	4·0	7·0	6·8	7·0	7·9
	19 21	62·0	3·9	7·3	7·0	7·5	8·1
	25 21	58·4	1·8	6·0	5·6	6·3	5·9
	27 21	62·0	4·0	3·7	3·7	3·7	5·0
Principally cloudy.	8 21	59·3	3·2	5·6	5·5	5·8	6·1
	30 21	59·5	7·0	8·4	8·2	8·8	8·0
Cloudy.	23 21	53·9	0·7	1·3	1·4	1·2	3·9
	29 21	61·0	2·3	1·2	1·7	2·2	3·8

Table LXXXIII.

General state of the sky during the night previous to reading the instruments.	1843. September. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed					
			On long grass exposed to $\frac{3}{4}$ths of sky.	On long grass.	On short grass.	One inch high.	Three inches high.	In focus of metallic reflector.
	d h							
Cloudless.	2 21	56·8	10·3	......	8·5	8·5	8·5	7·3
	5 21	48·1	6·1	......	5·1	7·1	9·1	7·5
	6 21	52·9	5·8	......	8·9	8·9	8·9	10·5
	9 21	58·0	6·7	......	11·8	12·3	12·0	11·0
	13 21	50·0	......	13·8	12·0	8·5	7·0	13·0
	19 21	53·3	......	12·3	13·3	9·3	5·8	10·8
	20 21	50·6	......	8·8	7·8	6·6	3·6	6·1
	22 21	50·8	......	10·8	11·5	8·8	5·2	10·3
	23 21	52·6	......	11·4	9·5	9·1	5·5	7·8
	24 21	47·4	......	9·9	10·6	8·1	4·5	8·1
	27 21	38·8	......	13·0	10·9	9·8	6·3	8·4
Principally cloudless.	4 21	44·3	......	......	9·0	9·1	9·8	9·0
	7 21	56·6	7·4	......	12·4	12·8	12·5	11·6
	12 21	51·8	......	......	10·8	10·8	10·9	9·5
	18 21	60·2	......	8·8	8·0	5·7	3·2	6·7
	26 21	39·5	......	8·7	8·5	7·3	3·3	7·5
	28 21	36·0	......	......	......	......	......	8·4
Half-cloudy.	1 21	58·6	5·3	......	8·3	8·3	8·8	8·9
	10 21	57·7	4·1	......	5·7	5·3	6·1	8·6
	15 21	58·0	......	10·8	9·3	6·0	4·0	7·0
	16 21	54·9	......	7·2	6·1	3·9	2·8	3·7
	17 21	57·8	......	8·8	7·3	5·8	3·6	3·8
	21 21	52·0	......	9·8	10·3	8·8	3·8	9·0
	29 21	47·8	......	10·4	9·7	7·3	4·8	9·7
Principally cloudy.	11 21	58·7	5·4	......	5·8	5·2	6·5	10·0
	14 21	61·0	......	......	4·2	2·0	4·0	2·5
	28 21	43·4	......	6·2	5·1	4·7	3·4	7·4
Cloudy.	3 21	57·2	5·7	......	3·7	4·2	4·6	7·2
	8 21	61·6	4·8	......	5·4	5·3	5·6	8·6
	30 21	55·8	......	−0·5	−0·4	−0·2	−0·9	−0·4

Table LXXXIV.

General state of the sky during the night previous to reading the instruments.	1843. October. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed									
			On long grass.	On short grass.	One inch high.	Three inches high.	Six inches high.	In focus of metallic reflector.	On white cotton wool.	On white raw wool.	On coarse flax.	On fine flax.
	d h	°	°	°	°	°	°	°	°	°	°	°
Cloudless.	16 21	32·3	7·8	7·2	5·8	......	5·2	8·6				
	18 21	28·5	10·0	9·0	8·5	......	......	11·0	12·3	14·0	13·0	17·3
	19 21	28·9	8·2	6·9	5·2	......	......	5·0	9·1	11·9	11·6	11·1
	21 21	37·5	7·3	5·6	4·5	......	......	5·3	7·0	10·0	8·0	9·5
	23 21	47·9	5·1	3·7	2·7	......	......	4·7	9·9	13·7	5·7	7·1
	25 21	29·3	7·5	6·3	5·7	......	......	6·1	9·3	10·3	10·3	10·8
	26 21	32·3	6·8	4·2	4·2	......	......	4·6	6·8	8·5	8·1	7·8
	29 21	40·1	11·2	8·1	6·3	......	......	7·6	11·3	13·6	13·4	13·4
Principally cloudless.	3 21	56·3	8·0	6·3	5·3	......	3·5	4·1				
	5 21	51·3	5·7	5·9	3·3	......	1·3	4·3				
	9 21	39·1	8·5	8·2	7·1	......	3·1	5·9				
	12 21	34·9	6·7	5·4	3·7	......	2·4	6·4				
	14 21	31·0	9·7	9·3	7·5	......	3·8	10·3				
	15 21	29·7	9·9	9·2	9·3	......	4·6	10·3				
	22 21	46·3	10·1	8·3	7·6	......	......	9·5	9·5	10·1	9·3	10·1
	28 21	34·5	5·7	4·2	3·2	......	......	5·7	7·7	7·5	8·5	8·5
Half-cloudy.	1 21	55·8	5·5	5·3	3·8	3·3	......	5·3				
	2 21	50·0	8·2	8·1	5·9	......	3·8	8·0				
	4 21	54·3	9·7	8·9	7·8	......	4·4	7·3				
	6 21	56·9	6·2	5·7	5·9	......	4·3	4·7				
	17 21	36·5	5·5	3·5	3·0	......	......	4·6	6·2	8·0	7·0	6·5
	24 21	46·5	2·9	2·0	2·3	......	......	2·3	1·7	2·7	2·6	2·7
Principally cloudy.	7 21	58·0	4·8	3·0	3·0	......	3·0	2·8				
	13 21	36·6	8·1	6·4	4·9	......	3·6	6·0				
	27 21	38·5	7·0	2·5	2·5	......	......	5·0	6·5	2·5	2·5	2·5
Cloudy.	8 21	48·5	......	......	......	......	......	0·9				
	10 21	46·9	0·7	0·9	0·9	......	1·7	0·7				
	11 21	42·3	7·3	5·0	3·8	......	2·3	6·4				
	20 21	38·3	3·3	0·6	−0·5	......	......	3·8	2·8	6·3	5·9	5·8
	30 21	51·1	6·9	1·8	1·6	......	......	1·6				
	31 21	42·5	0·3	0·2	0·0	......	......	1·0				

Table LXXXV.

General state of the sky during the night previous to reading the instruments.	1843. Nov. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed On long grass.	On short grass.	One inch high.	In focus of metallic reflector.	On white raw wool.	On white cotton wool.	On fine flax.	On coarse flax.
	d h									
Cloudless.	8 21	29·8°	10·7°	8·4°	6·3°	10·1°	14·8°	13·8°	13·0°	12·8°
	11 21	32·6	10·6	8·4	......	9·6	15·3	11·6	13·8	15·4
	12 21	27·4	11·5	8·4	......	8·2	13·9	11·2	12·9	14·2
	13 21	32·3	10·3	8·1	......	9·1	16·1	10·8	9·9	10·3
	16 21	31·0	8·9	7·8	......	6·2	......	......	9·2	8·9
	29 21	32·9	7·1	6·9	......	7·6	11·4	......	10·2	
Principally cloudless.	1 21	35·9	7·3	4·9	3·9	3·9	8·9	8·1	11·7	9·3
	4 21	43·0	7·8	5·2	4·8	6·0	11·0	7·4	9·2	10·8
	6 21	47·4	12·9	3·6	3·2	3·9	6·4	3·4	5·6	5·8
	7 21	41·5	8·5	8·3	5·5	6·3	11·5	9·5	10·5	10·0
	9 21	31·6	8·8	......	......	10·1	12·1	11·6	11·6	11·7
	18 21	34·6	5·8	5·4	......	5·8	5·8	......	6·2	
	28 21	44·4	10·9	9·6	......	9·4	11·2	......	9·2	
Half-cloudy.	3 21	47·8	5·8	4·8	3·8	5·3	9·3	6·8	8·6	8·6
	14 21	30·8	10·8	10·2	......	9·8				
	19 21	37·9	0·6	......	......	4·9	2·9			
	20 21	41·8	4·7	5·5	......	5·6	6·3	......	6·1	
	24 21	35·5	5·3	4·8	......	7·6	8·0	......	8·0	
	25 21	45·9	5·5	5·3	......	5·5	9·3	......	9·3	
	26 21	51·6	3·0	3·2	......	2·9	3·2	2·9		
Principally cloudy.	26 21	51·6	3·0	3·2	......	2·9	3·2	......	3·4	
	27 21	46·7	4·7	4·7	......	0·1	5·9	......	5·2	
Cloudy.	2 21	41·2	−1·3	−0·3	0·3	−0·1	0·6	−0·1	0·2	−0·1
	5 21	40·7	6·3	4·0	3·5	3·7	7·7	4·7	6·5	8·5
	15 21	31·6	1·6	0·3	......	1·8	3·4	1·3	−0·4	0·2
	17 21	37·7	2·5	2·7	......	2·7	3·5	2·5	3·7	3·1
	21 21	52·6	4·1	4·3	......	3·6	4·6			
	22 21	44·6	2·4	2·1	......	2·8	3·5			
	25 21	45·9	−0·3	−4·1	......	0·1	−0·1	......	−0·1	
	30 21	45·7	1·6	...	......	1·2				
Cloudy: cirro-cumulus	10 21	32·6	9·7	7·6	......	9·5	12·6	12·2	12·3	13·4

Table LXXXVI.

General state of the sky during the night previous to reading the instruments.	1843. Dec. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed On long grass.	On short grass.	On coarse flax.	On white raw wool.	On blackened tin.	On white tin.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°	°
Cloudless.	None.	......	......	......	......	......	......	......	......
Principally cloudless.	2 21	36·5	5·7	4·3	6·5	6·7	4·2	1·7	6·4
	6 21	40·2	5·9	6·2	7·2	7·2	5·4	4·2	7·2
	8 21	33·7	4·9	......	4·2	7·2	5·5	4·7	5·5
	10 21	39·0	10·2	9·0	10·2	10·0	9·2	8·0	7·2
	17 21	38·5	8·7	9·5	6·7	7·0	5·3	5·7	4·1
Half-cloudy.	5 21	40·1	7·1	6·1	7·3	6·9	5·9	5·9	7·9
Principally cloudy.	1 21	37·6	8·6	8·6	8·3	7·4	8·6	6·8	10·4
	7 21	45·7	6·2	4·7	5·7	6·9	4·2	3·4	3·8
	14 21	39·3	2·5	4·3	3·3	3·5	3·7	2·3	4·3
	15 21	43·1	6·3	4·9	6·3	5·1	4·9	......	5·1
	24 21	48·4	7·7	6·2	6·0	6·2	4·4	3·2	5·4
	25 21	42·7	8·9	8·3	10·6	10·7	7·8	6·0	7·5
	27 21	42·5	2·9	2·9	2·7	2·5	2·5	2·5	3·5
Cloudy.	3 21	45·3	2·3	3·3	2·0	2·1	......	......	3·3
	4 21	45·5	1·9	2·2	1·5	1·7	2·1	2·2	2·8
	9 21	35·1	2·5	−1·9	1·1	0·6	0·8	1·1	2·6
	11 21	30·4	1·1	1·4	2·2	0·3	0·4	0·2	2·9
	12 21	25·6	3·8	3·4	5·3	2·8	4·6	4·3	2·8
	13 21	39·3	6·6	7·8	7·1	8·3	6·5	5·3	2·3
	16 21	44·7	7·5	5·9	5·9	5·6	......	3·2	5·7
	18 21	42·5	2·5	3·2	3·1	2·4	2·5	2·7	3·7
	19 21	42·3	0·6	0·0	2·0	1·1	1·3	2·3	3·6
	20 21	41·4	3·9	2·4	2·2	1·9	1·9	1·9	3·2
	21 21	41·9	2·9	2·7	2·3	2·2	1·8	2·1	3·9
	22 21	42·5	0·5	1·2	0·9	−0·3	0·3	0·9	2·3
	23 21	49·8	5·6	4·8	5·0	4·2	4·0	3·8	4·5
	26 21	42·5	1·5	2·5	1·5	0·9	1·5	1·9	2·7
	28 21	42·5	1·8	2·0	1·6	1·9	1·2	1·4	2·3
	29 21	38·5	3·2	3·2	3·5	3·3	2·5	2·6	5·2
	30 21	39·3	2·1	2·5	2·8	2·1	2·1	2·5	3·7

Table LXXXVII.

General state of the sky during the night previous to reading the instruments.	1844. January. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed						
			On long grass.	On short grass.	On flax.	On white raw wool.	On blackened tin.	On white tin.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°	°
Cloudless.	2 21	18·6	8·8	......	......	......	......	......	8·3
	15 21	25·4	10·2	9·9	10·6	13·4	9·6	8·9	7·7
Principally cloudless.	7 21	32·2	8·0	7·0	7·2	7·7	7·0	6·2	3·2
	20 21	33·2	9·0	7·2	9·0	12·2	8·0	6·2	6·2
	21 21	39·2	7·4	7·2	9·2	11·2	7·4	6·2	3·6
	22 21	31·4	10·2	8·7	9·5	3·7	9·4	10·1	5·4
	24 21	30·5	8·5	7·5	9·0	11·5	8·5	8·0	6·0
	26 21	35·2	7·0	6·0	7·2	9·3	7·0	6·2	2·2
	28 21	37·3	9·5	8·5	9·4	10·1	8·4	8·1	5·6
Half-cloudy.	6 21	37·8	6·4	5·8	4·8	7·4	6·5	5·1	1·6
	14 21	28·3	7·9	5·7	6·0	10·3	7·3	5·3	7·3
	16 21	27·6	7·0	5·4	6·4	11·9	6·6	5·4	4·6
	18 21	39·2	7·0	6·1	4·2	6·9	7·0	5·4	2·7
	19 21	37·4	8·6	6·6	5·8	9·2	6·7	5·7	3·9
Principally cloudy.	5 21	45·8	5·6	4·5	3·6	3·5	4·8	2·8	1·8
	10 21	36·4	7·7	6·4	5·4	7·4	8·4	8·1	1·9
	11 21	37·7	8·5	8·7	7·9	11·7	9·5	9·3	3·3
	25 21	35·3	4·7	3·5	2·7	3·0	3·5	3·5	1·6
	27 21	40·6	7·3	5·6	6·5	6·2	5·8	5·1	2·1
	29 21	39·7	8·3	8·0	7·1	8·5	7·5	7·4	3·9
	31 21	27·7	5·9	6·4	5·7	8·9	7·2	7·7	3·9
Cloudy.	3 21	22·4	2·9	......	3·4	3·4	......	......	3·4
	8 21	34·2	7·6	6·4	5·6	7·5	7·2	7·2	1·8
	9 21	30·7	1·7	0·9	0·2	1·7	0·9	0·5	0·2
	12 21	39·2	1·8	1·7	1·2	2·4	2·4	1·9	−0·8
	13 21	32·7	1·6	0·9	0·7	1·5	1·7	1·2	−1·8
	17 21	36·5	4·5	4·2	3·3	3·9	4·5	1·7	0·5
	23 21	36·2	4·6	4·2	4·6	5·9	4·9	3·9	2·4
	30 21	32·3	3·3	2·3	1·2	2·7	2·9	2·3	−0·3

Table LXXXVIII.

General state of the sky during the night previous to reading the instruments.	1844. February. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed						
			On long grass.	On short grass.	On flax.	On white raw wool.	On blackened tin.	On white tin.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°	°
Cloudless.	1 21	27·2	8·6	5·7	6·6	9·0	7·6	8·0	6·4
	15 21	33·0	11·6	8·8	12·0	9·8	9·2	10·9	7·8
	16 21	34·0	7·7	6·5	6·9	6·7	6·0	5·0	6·2
	22 21	22·2	10·4	7·5	10·8	9·8	7·7	7·0	7·7
Principally cloudless.	4 21	29·0	7·5	6·0	6·0	10·2	10·0	10·0	6·0
	6 21	25·4	8·2	6·6	11·6	6·8	8·4	6·6	7·1
	7 21	32·2	5·5	5·0	9·7	6·0	5·2	4·7	3·4
	8 21	32·7	7·1	5·5	5·3	8·7	6·4	6·5	3·6
	19 21	29·7	8·4	6·2	7·8	6·7	5·8	7·0	6·7
	20 21	27·3	8·5	5·3	6·7	9·0	6·3	6·1	6·3
Half-cloudy.	17 21	36·7	7·5	5·3	5·2	3·7	4·7	4·7	4·5
	18 21	40·6	5·4	6·2	4·0	2·8	5·1	1·5	3·8
	24 21	33·4	8·2	5·8	7·2	8·6	6·9	6·4	6·4
	25 21	39·5	6·6	5·2	4·9	3·7	5·5	5·9	3·0
	27 21	29·2	3·8	1·0	4·4	2·7	3·1	2·9	1·2
	28 21	32·2	5·9	3·9	4·2	3·7	5·1	5·6	3·2
Principally cloudy.	6 21	32·0	7·2	5·7	9·0	8·0	6·2	5·7	3·2
	9 21	32·2	2·8	2·8	4·5	3·6	4·4	4·0	1·4
	10 21	31·7	6·5	4·5	4·7	4·7	5·2	4·7	3·9
	12 21	24·5	6·2	3·5	9·6	7·0	4·5	3·7	4·3
	13 21	20·0	6·0	2·0	11·0	12·2	4·0	5·2	3·4
	26 21	27·0	4·9	1·8	5·7	5·4	4·8	4·5	2·6
Cloudy.	2 21	28·0	3·0	2·0	4·0	3·0	6·5	7·0	3·0
	3 21	28·0	2·7	4·3	4·0	3·0	10·0	6·0	2·2
	11 21	30·2	6·0	6·0	4·1	7·2	5·2	4·7	4·7
	14 21	36·8	8·5	7·6	13·8	14·8	7·3	6·8	7·5
	21 21	32·0	0·2	0·3	0·5	−0·2	1·4	0·4	−1·5
	23 21	31·2	4·2	3·2	4·8	3·7	4·4	4·0	5·2
	29 21	38·8	3·0	1·6	4·3	1·5	3·4	3·1	1·8

Table LXXXIX.

General state of the sky during the night previous to reading the instruments.	1844. March. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed					
			On long grass.	On short grass.	On flax.	On white raw wool.	On lead.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°
Cloudless.	5 21	24·1	8·9	7·9	14·5	14·9	8·5	12·1
	6 21	30·6	6·8	4·1	4·8	4·6	5·9	5·1
	12 21	31·1	7·1	4·1	5·6	6·8	5·0	4·1
	20 21	27·9	11·4	8·4	18·1	18·9	6·4	8·9
	21 21	32·0	7·2	6·3	12·8	12·4	4·7	5·3
	23 21	33·3	8·3	5·8	9·3	8·3	5·1	4·1
	28 21	34·9	8·7	7·1	13·4	12·6	5·7	5·9
Principally cloudless.	25 21	44·0	6·8	5·6	6·0	7·0	5·8	4·4
	30 21	39·8	6·3	5·5	9·0	8·7	4·5	5·4
Half-cloudy.	1 21	37·1	5·4	4·6	4·0	2·1	5·8	3·1
	3 21	38·6	5·6	3·4	4·6	4·9	4·2	2·1
	7 21	29·6	10·8	6·8	12·0	14·2	8·6	9·4
	10 21	31·7	5·4	3·7	7·7	7·4	4·2	2·7
	11 21	32·8	2·8	2·8	……	……	……	0·0
	13 21	33·7	7·5	5·9	7·6	8·7	5·9	5·5
	17 21	30·3	5·7	4·0	5·5	6·1	4·8	5·1
	24 21	39·5	7·6	5·8	5·8	3·5	6·5	2·0
	29 21	37·9	7·9	5·1	7·9	6·6	2·7	9·3
Principally cloudy.	9 21	40·3	4·3	3·8	7·3	7·3	4·3	1·8
	15 21	36·0	6·6	5·8	6·8	7·9	6·5	5·6
	18 21	36·1	8·1	5·5	8·1	9·1	5·4	5·4
	19 21	39·5	7·3	6·0	6·5	7·2	6·5	4·5
	27 21	42·5	6·5	4·6	8·0	8·5	3·7	3·6
Cloudy.	2 21	35·3	4·3	4·3	4·3	4·3	5·3	2·3
	4 21	32·4	5·9	4·2	5·2	5·6	5·3	5·9
	8 21	36·2	4·6	3·0	3·1	3·8	3·0	3·5
	14 21	38·1	0·5	1·1	2·8	0·9	1·7	−0·2
	16 21	37·5	0·1	0·5	1·1	0·5	0·7	−1·0
	22 21	39·8	1·3	1·1	2·5	1·5	1·1	−1·2
	26 21	46·1	8·1	7·7	9·1	9·1	6·3	4·1
	31 21	37·3	0·6	0·2	2·0	2·4	−0·2	0·7

Table XC.

General state of the sky during the night previous to reading the instruments.	1844. April. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed					
			On long grass.	On short grass.	On flax.	On white raw wool.	On lead.	In focus of metallic reflector.
	d h	°	°	°	°	°	°	°
Cloudless.	1 21	36·4	7·9	8·4	13·2	11·2	6·7	6·8
	2 21	41·6	12·9	13·4	19·4	17·6	12·9	9·4
	3 21	43·4	13·4	11·4	19·9	18·9	16·4	8·4
	4 21	44·4	11·4	11·1	14·4	14·4	6·9	5·4
	5 21	40·4	11·4	9·2	14·6	13·4	4·6	7·3
	6 21	34·4	10·9	8·9	13·6	12·4	6·4	5·1
	7 21	33·4	11·3	11·8	15·4	16·9	7·9	10·7
	8 21	39·4	10·2	9·9	16·2	15·8	8·4	6·1
	9 21	41·2	11·7	10·7	13·4	13·2	8·7	6·5
	10 21	38·3	13·1	11·6	18·0	16·8	9·5	8·7
	16 21	41·9	10·7	9·7	15·3	15·0	7·8	5·5
	17 21	44·6	11·1	10·6	16·6	15·3	8·6	5·3
	18 21	37·0	12·6	10·1	15·3	14·4	10·0	9·7
	24 21	39·4	14·9	13·6	19·9	20·4	12·4	12·4
	25 21	41·4	13·4	12·6	19·2	17·8	10·4	10·2
	28 21	35·7	11·7	11·4	18·1	15·4	9·5	10·7
	29 21	39·4	10·4	8·7	14·2	12·6	9·0	9·4
	30 21	40·7	12·5	10·5	14·4	14·2	10·2	6·3
Principally cloudless.	19 21	48·2	11·0	10·1	15·1	12·6	12·2	7·0
	22 21	45·1	12·1	10·9	19·1	16·5	13·6	8·3
	23 21	41·4	8·4	6·4	10·4	11·5	6·2	6·2
	27 21	37·1	9·8	9·2	13·4	12·0	8·5	8·1
Half-cloudy.	14 21	43·9	5·7	5·4	8·7	8·6	4·8	3·1
	15 21	48·6	6·6	6·1	8·4	8·5	4·0	4·2
	21 21	43·6	6·4	6·6	9·4	7·6	5·6	2·8
Principally cloudy.	11 21	42·0	10·0	8·8	14·0	14·0	6·8	7·8
	20 21	51·1	8·9	9·0	12·3	10·5	11·1	6·8
Cloudy.	12 21	46·4	2·4	2·5	5·1	6·0	2·1	2·1
	13 21	48·7	4·9	5·1	8·4	7·7	5·3	3·1
	26 21	46·1	0·6	0·1	1·3	2·0	0·4	1·1

TABLE XCI.

General state of the sky during the night previous to reading the instruments.	1844. May. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed On long grass.	On short grass.	On flax.	On white raw wool.	On lead.	In focus of metallic reflector.
	h h	°	°	°	°	°	°	°
Cloudless.	1 21	40·9	12·3	12·3	16·1	12·8	10·9	7·3
	2 21	40·9	14·7	10·9	17·6	14·6	10·2	9·3
	3 21	48·0	11·8	9·7	12·0	13·0	7·8	7·8
	6 21	44·6	11·6	10·6	14·1	13·1	......	9·1
	8 21	49·6	9·8	12·4	13·6	12·4	......	8·6
	10 21	45·2	13·5	12·2	13·4	12·9	......	7·7
	15 21	44·6	15·6	13·6	16·4	9·4	9·3	12·8
	23 21	44·2	7.5	6·1	10·8	10·3	......	4·5
	25 21	40·6	9·4	9·1	11·1	11·4	......	7·1
	27 21	42·1	6·8	9·4	6·9	9·1	......	6·3
Principally cloudless.	5 21	44·6	9·6	11·6	11·9	11·4	9·3	4·2
	13 21	46·8	12·8	11·8	14·0	16·8	......	7·8
	17 21	33·9	7·4	6·1	5·7	13·9	......	6·9
	19 21	43·2	13·2	12·2	......	......	......	6·4
	20 21	43·8	6·0	5·8	5·9	3·3	1·0	4·9
Half-cloudy.	7 21	46·7	11·7	10·7	......	......	......	8·2
	12 21	44·4	8·4	7·4	......	......	......	5·6
	16 21	44·4	12·4	10·4	14·5	14·5	7·4	8·4
	18 21	39·7	7·1	7·7	11·5	11·7	5·7	7·5
	21 21	37·4	2·4	1·3	2·3	2·4	0·5	−2·0
	26 21	42·0	8·7	8·5	......	......	......	7·8
Principally cloudy.	24 21	46·2	5·2	4·7	8·7	8·7	......	−1·3
Cloudy.	4 21	48·4	2·4	1·4	......	......	......	−1·6
	9 21	51·7	10·7	9·9	9·4	9·3	......	11·4
	11 21	51·4	3·4	2·4	......	......	......	2·2
	14 21	42·4	9·4	8·4	......	......	......	5·4
	22 21	47·4	0·2	0·0	......	......	......	0·4
	28 21	46·8	3·3	4·3	4·1	4·8	......	3·5
	29 21	45·2	5·2	5·7	......	......	......	2·4
	30 21	45·9	3·4	3·8	4·6	4·9	......	3·4
	31 21	44·5	11·9	12·5	15·5	15·7	......	9·8

Table XCII.

General state of the sky during the night previous to reading the instruments.	1844. June. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed On long grass.	On short grass.	On flax.	On white raw wool.	In focus of metallic reflector.
	d h	°	°	°	°	°	°
Cloudless.	10 21	48·1	10·8	10·1	9·5	9·6	5·1
	13 21	53·0	8·6	8·8	7·0	9·0	7·0
	16 21	47·1	10·9	9·4	5·7	5·6	2·5
	22 21	54·0	11·2	10·1	......	......	10·0
	23 21	63·1	12·2	7·1	9·1	9·1	6·6
	30 21	52·5	11·4	10·4	......	......	9·2
Principally cloudless.	1 21	44·1	8·1	8·1	10·1	9·3	7·9
	4 21	48·6	9·6	11·6	12·9	13·6	9·1
	8 21	52·8	10·5	11·2	12·3	......	8·3
	14 21	49·2	7·9	7·0	7·2	7·9	6·0
	21 21	57·3	8·1	7·2	9·5	10·4	7·4
Half-cloudy.	3 21	48·8	10·8	10·8	......	......	7·9
	7 21	52·4	8·4	7·6	8·9	......	4·6
	9 21	51·3	6·5	6·1	6·5	......	4·8
	11 21	49·5	6·0	5·5	......	......	3·5
	17 21	53·9	10·7	11·5	6·9	8·8	4·1
	20 21	52·9	3·7	4·6	4·2	6·9	3·4
	29 21	58·9	7·9	7·1	......	......	6·9
Principally cloudy.	2 21	44·5	12·5	12·0	......	......	9·3
	5 21	54·2	6·6	6·2	8·4	6·4	5·0
	6 21	54·6	4·6	5·4	5·7	6·6	3·0
	12 21	52·7	7·4	6·3	7·2	8·9	0·2
	15 21	46·9	12·3	11·5	......	......	9·5
	18 21	52·5	9·7	8·2	10·3	11·2	7·3
	19 21	47·9	5·9	5·9	7·9	7·9	5·1
	24 21	58·9	6·8	6·0	7·9	7·9	1·9
	27 21	51·7	12·7	8·9	12·5	12·7	10·6
	28 21	49·9	8·1	8·2	12·1	11·6	8·1
Cloudy.	25 21	47·7	1·4	1·6	2·4	1·7	3·5
	26 21	52·9	1·9	1·6	2·6	1·9	3·9

TABLE XCIII.

General state of the sky during the night previous to reading the instruments.	1844. July. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed				
			On long grass.	On short grass.	On flax.	On white raw wool.	In focus of metallic reflector.
	d h	°	°	°	°	°	°
Cloudless.	13 21	59·0	8·0	6·0	......	......	6·5
	14 21	51·2	8·2	6·2	......	......	6·7
	15 21	54·5	14·2	12·8	16·8	11·5	12·9
	16 21	47·1	7·8	6·8	9·1	9·6	4·9
	18 21	47·3	11·8	10·3	16·1	13·3	7·3
	19 21	48·0	9·7	10·1	13·8	14·2	7·4
	20 21	49·5	10·0	8·5	12·0	12·0	7·5
	22 21	57·3	12·1	10·3	14·1	14·1	11·3
	23 21	60·2	10·2	8·6	11·4	11·9	7·7
	24 21	57·0	14·7	10·2	12·9	13·1	8·5
	27 21	53·3	10·3	8·2	11·3	11·3	7·3
	29 21	49·3	13·3	11·3	......	......	10·0
Principally cloudless.	21 21	53·8	8·8	10·0	13·1	12·9	7·1
Half-cloudy.	5 21	51·1	8·6	6·1	9·9	10·0	7·0
	7 21	55·0	8·0	7·2	......	......	3·7
	8 21	57·1	5·3	5·2	3·5	6·3	4·8
	9 21	51·7	9·2	8·7	11·5	7·9	7·9
	26 21	57·9	17·3	......	......	......	7·9
Principally cloudy.	1 21	55·0	8·0	7·0	8·0	8·0	3·4
	3 21	54·6	7·6	7·0	8·1	8·1	5·0
	4 21	55·5	6·5	6·0	......	......	3·5
	11 21	56·2	5·2	4·9	5·2	5·2	4·4
	12 21	54·2	7·2	6·2	7·2	7·2	3·2
	17 21	54·5	4·5	3·5	5·3	4·5	6·5
	25 21	62·0	15·3	15·4	12·5	19·0	5·5
	28 21	58·1	8·1	7·1	......	......	3·8
	31 21	48·7	5·2	7·7	7·7	7·7	7·7
Cloudy.	2 21	52·5	3·9	2·4	3·5	2·9	2·9
	6 21	55·0	2·8	2·0	3·8	3·8	3·2
	10 21	60·5	3·5	2·0	......	......	1·5
	30 21	56·8	2·7	2·3	3·5	3·3	5·0

Table XCIV.

General state of the sky during the night previous to reading the instruments.	1844. August. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed				
			On long grass.	On short grass.	On flax.	On white raw wool.	In focus of metallic reflector.
	d h	°	°	°	°	°	°
Cloudless.	1 21	45·6	7·9	5·5	7·1	6·8	6·6
	2 21	52·8	11·3	9·8	11·3	12·6	8·2
	7 21	50·9	4·3	5·6	6·7	6·5	7·3
	8 21	49·2	6·2	6·7	8·6	8·7	7·4
	13 21	52·8	11·6	5·3	6·6	6·3	4·1
	14 21	49·0	8·0	5·5	8·5	8·0	4·9
	18 21	45·4	8·6	6·9	8·4	11·1	6·7
	27 21	42·8	11·0	10·0	12·0	......	8·0
	28 21	44·6	9·3	8·8	12·5	......	7·8
	29 21	46·8	10·0	10·1	13·9	......	9·5
	30 21	46·2	10·5	9·3	11·9	......	7·5
	31 21	46·3	8·5	7·8	10·6	......	6·7
Principally cloudless.	4 21	49·7	9·5	5·3	7·7	8·4	9·4
	6 21	52·3	6·3	5·3	5·8	6·3	6·5
	15 21	50·3	7·7	4·8	7·4	6·8	7·8
	17 21	47·3	7·1	6·1	9·6	7·1	6·9
	22 21	48·2	8·3	6·4	9·4	8·5	6·7
	23 21	47·5	9·7	8·3	10·5	11·7	7·8
Half-cloudy.	3 21	54·2	4·6	4·5	16·6	14·0	7·2
	9 21	47·8	7·8	6·3	7·8	7·8	8·6
	10 21	45·3	11·1	8·1	12·3	11·3	8·8
	16 21	57·2	......	......	......	......	5·4
	26 21	51·2	10·0	6·7	10·9	......	7·7
Principally cloudy.	20 21	51·6	6·4	5·3	5·9	5·8	7·9
Cloudy.	5 21	57·8	10·8	11·6	11·6	13·6	3·3
	11 21	56·2	4·2	4·2	......	......	3·7
	12 21	54·5	3·2	3·0	4·8	5·8	5·4
	19 21	57·7	0·9	0·7	0·9	1·0	1·1
	21 21	52·8	3·0	2·6	6·0	3·5	3·8
	24 21	52·3	7·6	5·8	7·3	8·5	6·5
	25 21	52·4	4·4	3·2	5·3	......	9·1

Table XCV.

General state of the sky during the night previous to reading the instruments.	1844. September. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed On long grass.	On short grass.	On flax.	In focus of metallic reflector.
	d h	°	°	°	°	°
Cloudless.	1 21	48·7	11·9	11·2	14·4	12·2
	4 21	59·4	6·3	5·9	8·1	7·9
	10 21	44·8	10·8	10·0	12·8	11·3
	18 21	41·3	11·3	8·3	11·3	7·8
	19 21	47·3	11·5	9·6	13·6	13·7
	20 21	45·9	11·1	10·9	14·6	10·1
	24 21	41·8	11·5	9·6	11·8	9·6
	25 21	40·8	10·5	8·4	12·6	9·6
	26 21	41·1	11·1	8·5	14·9	9·5
	27 21	40·3	9·9	6·1	......	8·1
	28 21	45·1	9·8	8·1	......	8·5
	29 21	34·8	9·8	7·3	12·2	7·6
Principally cloudless.	2 21	51·0	10·5	9·5	13·7	8·4
	5 21	57·0	10·0	9·7	12·4	8·3
	11 21	49·8	9·6	8·3	10·6	10·0
	21 21	47·1	10·6	10·1	13·1	11·1
	22 21	49·1	13·1	12·1	......	5·1
Half-cloudy.	6 21	62·6	......	......	......	16·2
	7 21	56·8	6·3	5·6	8·7	7·8
Principally cloudy.	12 21	52·9	7·5	5·7	8·6	8·2
	13 21	53·8	6·8	6·4	7·5	8·1
	16 21	60·8	5·4	4·6	6·5	5·0
	17 21	53·9	5·3	5·2	7·8	2·3
	23 21	49·3	7·0	3·9	7·4	8·1
Cloudy.	3 21	59·6	3·8	3·1	4·2	3·4
	8 21	55·5	3·2	1·5	3·2	6·0
	9 21	55·8	0·8	1·1	0·8	5·8
	14 21	57·3	4·8	4·4	4·9	6·0
	15 21	61·4	4·1	4·6	4·8	5·9
	30 21	42·2	1·6	2·6	2·9	

Table XCVI.

General state of the sky during the night previous to reading the instruments.	1844. October. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed			
			On long grass.	On short grass.	On flax.	In focus of metallic reflector.
	d h	°	°	°	°	°
Cloudless.	3 21	53·3	18·8	17·8	21·8	13·7
	7 21	33·2	10·2	8·7	19·5	4·2
	10 21	47·2	7·7	7·2	8·6	3·2
	18 21	36·5	6·7	5·3	7·5	4·2
	22 21	30·8	6·5	4·6	7·3	5·3
	26 21	43·2	12·5	9·0	13·7	7·4
	27 21	36·2	12·0	9·7	9·9	5·5
	30 21	40·8	11·0	7·6	10·6	5·6
Principally cloudless.	4 21	53·8	13·0	12·0	14·8	8·5
	5 21	44·0	10·2	9·0	13·0	3·5
	11 21	46·8	7·8	7·3	9·5	4·6
	14 21	47·5	6·4	5·5	8·0	7·0
	28 21	39·8	11·4	9·4	10·1	10·2
	31 21	44·2	8·2	5·5	7·0	6·7
Half-cloudy.	2 21	51·0	9·0	8·4	12·0	6·0
	6 21	44·1	8·7	8·1	8·1	4·0
	8 21	45·7	8·5	7·7	8·2	5·7
	13 21	49·2	7·9	6·2	12·2	7·2
	16 21	42·2	6·2	5·5	7·7	4·7
	17 21	39·8	5·4	4·5	7·8	8·3
Principally cloudy.	19 21	45·3	5·6	5·8	8·8	6·3
Cloudy.	1 21	57·6	5·1	4·6	5·6	3·2
	9 21	53·6	4·8	4·1	6·6	3·3
	12 21	55·8	5·7	6·0	9·8	3·6
	15 21	46·7	2·0	1·7	2·2	2·9
	20 21	44·5	10·0	6·4	11·5	7·9
	21 21	43·2	3·2	1·3	1·2	3·2
	23 21	35·5	7·7	9·7	13·3	5·0
	24 21	47·2	1·1	0·5	1·9	2·7
	25 21	43·1	9·1	5·5	16·1	7·9
	29 21	42·7	3·7	2·4	2·7	7·7

Table XCVII.

General state of the sky during the night previous to reading the instruments.	1844. November. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed			
			On long grass.	On short grass.	On flax.	In focus of metallic reflector.
	d h	°	°	°	°	°
Cloudless.	15 21	48·3	9·9	5·0	7·6	6·1
	20 21	34·8	10·3	5·8	11·8	5·8
Principally cloudless.	4 21	34·6	9·0	7·6	6·6	9·6
	13 21	43·4	10·8	8·9	9·4	6·4
	21 21	34·3	10·8	7·3	10·9	8·8
	23 21	33·5	11·0	8·8	11·5	9·5
	26 21	27·4	8·1	5·6	9·4	7·2
	27 21	29·5	7·5	4·8	9·5	5·0
	30 21	31·9	10·2	8·4	13·4	10·5
Half-cloudy.	6 21	38·3	9·3	7·6	8·3	7·3
	8 21	47·3	8·3	7·4	8·3	7·6
	9 21	38·3	4·9	4·6	7·0	6·3
	10 21	41·0	7·5	6·4	7·5	7·7
	24 21	36·6	9·4	6·0	10·7	8·6
	25 21	32·1	8·7	6·4	11·4	7·8
Principally cloudy.	3 21	36·8	7·0	4·0	6·1	8·0
	11 21	40·3	6·0	3·0	6·8	4·8
	16 21	49·0	5·0	4·4	6·4	4·2
Cloudy.	1 21	41·0	0·4	0·2	0·3	2·0
	2 21	37·0	1·0	0·1	0·5	2·5
	5 21	39·1	5·1	2·6	4·1	2·2
	7 21	43·9	3·4	2·6	3·1	4·6
	12 21	48·8	0·8	0·9	—0·1	1·8
	14 21	47·6	5·2	5·2	1·8	2·8
	17 21	48·7	0·8	0·6	0·6	2·7
	18 21	49·5	2·0	1·3	1·4	2·5
	19 21	46·4	2·9	1·9	1·1	4·5
	22 21	36·4	10·0	8·7	11·1	2·2
	28 21	35·5	3·8	3·0	3·5	4·6
	29 21	37·6	3·5	3·4	2·6	

Table XCVIII.

General state of the sky during the night previous to reading the instruments.	1844. December. Day and hour of reading.	Reading of thermometer in air at the height of 4 feet.	Excess of the reading of a thermometer in air, at the height of 4 feet and protected from the effects of radiation, above that placed			
			On long grass.	On short grass.	On flax.	In focus of metallic reflector.
	d h	°	°	°	°	°
Cloudless.	4 21	26·1	12·4	8·5	14·9	7·3
	5 21	21·4	13·5	7·4	17·4	13·7
	6 21	21·1	12·2	8·7	14·1	10·6
	7 21	24·5	9·6	5·9	10·0	10·1
	11 21	22·9	10·0	6·5	10·4	6·5
	19 21	31·3	8·0	6·0	6·8	7·8
Principally cloudless.	20 21	30·1	7·2	5·7	7·1	8·2
Half-cloudy.	None.	......	......	......	......	......
Cloudy.	1 21	34·1	1·9	0·8	2·1	3·9
	2 21	32·3	3·0	0·4	2·3	5·1
	3 21	34·1	2·2	1·1	1·6	3·6
	8 21	25·1	2·3	0·8	−0·4	3·9
	9 21	27·8	1·7	0·3	8·5	3·3
	10 21	27·7	1·2	−0·2	2·7	3·0
	12 21	23·2	0·7	−0·8	0·4	2·9
	13 21	24·4	−0·3	−0·9	2·9	2 2
	14 21	28·1	2·8	1·4	2·3	2·7
	15 21	35·4	5·1	3·6	4·6	4·8
	16 21	37·1	1·1	0·4	0·3	2·9
	17 21	38·1	2·0	1·1	1·1	3·1
	18 21	40·3	0·8	0·3	0·3	2·3
	22 21	29·1	4·9	2·9	4·1	5·9
	23 21	29·5	2·4	0·7	1·5	4·0
	24 21	31·3	2·3	1·8	2·3	2·3
	25 21	31·1	1·8	0·8	1·3	3·4
	26 21	27·6	8·8	3·4	8·3	0·4
	27 21	29·8	3·6	0·9	3·8	2·6
	28 21	40·5	1·7	1·0	1·3	3·2
	29 21	37·5	0·5	−1·1	−0·5	2·2
	30 21	34·1	5·8	3·4	2·5	5·9
	31 21	35·1	4·9	2·7	2·9	3·9

From Tables LI. to XCVIII. we learn the following particulars. As the series of observations of the thermometer whose bulb was placed in the focus of the parabolic reflector extends over a much longer period of time than any of the others, it is desirable to discuss the results derived from it before consulting those deduced from the observations of the other instruments. Confining ourselves therefore to the numbers found from the observations of the thermometer in the reflector; first, to those in that division of every table under cloudy nights, it will be seen that during such nights its reading has frequently been higher than that in air, but it has generally been from 0° to 3° lower; frequently 4°; less frequently from 5° to 8°, and occasionally 9° less than that in air. In Tables LXXII., LXXIV. and LXXV., quantities equal to 9° are exhibited; and in the last-mentioned table there is one case of 13°·3; in Table XCI. there is one case of 11°·4; during the nights that these large differences have occurred the clouds have been noted as being fleecy, thin and high. In Table LXXXV. large differences are exhibited; the sky was noted at the time as being covered with cirrocumuli.

As the results on those nights during which more or less cloud prevailed are generally between those on cloudy and on cloudless nights, and nearly proportional to the amount of cloud; the next division of these tables, which deserves particular attention, is that containing the results derived from cloudless nights. By examining the numbers in this division, it will be found that quantities less than 5° have taken place, even on cloudless nights. The atmosphere, however, on such nights has been noticed as being thick, and the air as being saturated with moisture. The following is a list of these cases:—

The result deduced from the observation taken in the year . . . .

1841, on March 10, at 21^{h}.
1841, on March 29, at 21.
1841, on June 20, at 21.
1841, on July 9, at 21.
1841, on September 6, at 21.
1841, on November 19, at 21.
1842, on April 8, at 21.
1842, on August 3, at 21.
1842, on August 16, at 21.
1842, on August 17, at 21.
1842, on August 28, at 21.
1842, on December 5, at 21.
1843, on October 26, at 21.
1843, on December 17, at 21.
1844, on March 12, at 21.
1844, on March 23, at 21.
1844, on May 20, at 21.
1844, on June 16, at 21.

The result deduced from the observation taken in the year . . . .
- 1844, on July 16, at 21^h.
- 1844, on August 13, at 21.
- 1844, on August 14, at 21.
- 1844, on October 7, 21.
- 1844, on October 10, at 21.
- 1844, on October 18, at 21.

In all twenty-five instances out of 331 cloudless nights; on one of these nights, viz. that preceding 1842, August 28, 21^h the result was a reading of $0°\cdot6$ higher than that of the thermometer in air, but it is very probable that the reading was in error.

The general amount on cloudless nights is from 7° to 10°; and frequently above 10°. During the four years' observations there were thirty-three cases in which the reading of the thermometer in air exceeded that of the thermometer in the reflector by quantities between 10° and 11°; there were nineteen cases between 11° and 12°; sixteen between 12° and 13°; eight between 13° and 14°; and there were four instances in which the excess amounted to 14° and was less than 15°; these cases occurred during the nights preceding 1842, June 16, 21^h and October 29, 21^h; 1843, February 12, 21^h and March 6, 21^h. (The differences in 1843 March were unusually large.) There were two cases exceeding 15°, both in 1842 on June 6 and 7; and there is one above 17° which occurred during the night preceding 1843 March 2, 21^h. This is the largest difference shown during the four years' observations, between the thermometer in the air and that in the reflector. I now proceed to speak of the results derived from the observations of the other instruments.

Those from cloudy nights with all the other thermometers were nearly the same as those with the thermometer with its bulb placed in the focus of the reflector, excepting that their readings were much less frequently higher than those of the thermometer in air than were those of that instrument.

On partially cloudy, and on cloudless nights, the reading of the thermometers on grass, those placed one inch above it, those on raw wool and flax, were always much lower than that in the reflector; and frequently, even on grass, the reading was as much below that in the reflector as the latter was below that in the air; on wool and flax it was nearly always so.

The greatest difference between the readings of that in air and that on short grass was $17°\cdot8$; and on long grass was $18°\cdot8$; and they took place during the night preceding 1844, October 3^d 21^h (see Table XCVI.). The reading of the thermometers which were placed on wool and flax were frequently from 15° to 18°, and occasionally from 19° to 21° less than that in air. The greatest difference with raw wool was $20°\cdot4$; and with flax was $21°\cdot8$, as observed 1844, April 24^d at 21^h, and on October 3^d at 21^h respectively.

The readings of the thermometer placed on garden mould were always (excepting when the sky was quite cloudy) nearer to that in the air than any of the other

readings mentioned in this section ; and this reading was nearer and nearer to that in the air in proportion to the looseness of the mould ; the heat from beneath passing the more rapidly to the surface in proportion to its looseness. It was also observed that the temperature about the roots of plants was lower in exact amount to the excess of heat conducted to the surface.

It must be borne in mind that the differences exhibited between the minimum readings of self-registering thermometers placed upon substances and fully exposed to the sky, and those of a similar thermometer placed in the air, and protected as much as possible from the sky, are not the maximum differences, unless the two minima occur at the same time ; this generally is not the case ; that of a thermometer placed on any substance and exposed to the sky usually occurs in the evening, or before midnight ; whilst that in the air occurs at about the time of the rising of the sun. The difference between these two minima should be increased by the difference between the readings of the thermometer in the air at those times ; this may amount to 10° ; and it is highly probable that during a portion of the evening or night, which precedes the reading of the instruments, at times when 20° are exhibited between the minima, a difference of 30° has existed.

From the preceding remarks it is evident that the differences between the readings of the thermometer in air and the others, have varied with every variation of the amount of cloud ; and by comparing the numbers in one table with those in another, it will be found that this difference is about the same in amount, with the same quantity of cloud at all times of the year.

This will be more clearly seen in the next table containing the monthly means of all the numbers contained in Tables LI. to XCVIII. of the results of the observations of the thermometer in the reflector.

Table XCIX.—Mean Monthly excess of the readings of the thermometer in Air above that in the Reflector.

General state of the sky during the night previous to reading the instruments.	Month.	The mean monthly excess, according to the state of the sky with respect to the quantity of cloud, of the readings of a self-registering thermometer placed in air at the height of 4 feet and protected from radiation, above those of a similar thermometer placed in the focus of a metallic parabolic reflector. 1841. Mean excess.	1841. No. of nights.	1842. Mean excess.	1842. No. of nights.	1843. Mean excess.	1843. No. of nights.	1844. Mean excess.	1844. No. of nights.	1841 Feb. 10, to 1844 Dec. 31. No. of nights.	1841 Feb. 10, to 1844 Dec. 31. Sum of excess.	1841 Feb. 10, to 1844 Dec. 31. Mean excess.
		°		°		°		°			°	°
Cloudless.	January.	......	......	9·4	6	9·5	8	8·0	2	16	148·7	9·3
	February.	5·8	1	8·1	4	11·0	5	7·0	4	14	121·4	8·7
	March.	7·1	12	6·5	8	10·2	15	6·5	7	42	336·2	8·0
	April.	9·4	7	7·1	9	10·1	11	8·0	18	45	384·9	8·1
	May.	7·2	7	8·5	8	10·5	2	8·1	10	27	219·7	8·1
	June.	8·8	4	10·2	12	10·3	7	6·8	6	29	267·5	9·2
	July.	7·4	4	8·7	6	10·1	2	8·2	12	24	199·9	8·3
	August.	9·9	4	6·2	11	9·1	6	7·1	12	33	247·6	7·5
	September.	8·1	6	4·5	3	9·2	11	9·7	12	32	279·0	8·7
	October.	8·7	5	9·1	9	6·6	8	6·8	8	30	227·7	7·6
	November.	8·1	9	10·6	3	8·5	6	6·0	2	20	167·7	8·4
	December.	8·7	7	6·6	6	......	0	9·3	6	19	156·0	8·2
Principally cloudless.	January.	......	......	6·2	7	7·3	3	4·6	7	17	97·2	5·7
	February.	......	0	5·8	5	7·2	2	5·5	6	13	76·5	5·9
	March.	6·1	5	6·6	3	8·4	5	4·9	2	15	101·6	6·8
	April.	5·8	6	5·1	7	7·4	6	7·4	4	23	144·4	6·3
	May.	7·0	4	6·7	10	7·8	4	6·0	5	26	179·7	6·9
	June.	7·4	6	6·4	8	7·6	6	7·7	5	26	187·6	7·2
	July.	4·7	3	6·5	13	8·4	10	7·1	1	26	180·9	6·9
	August.	5·6	6	5·7	4	8·1	12	7·5	6	28	198·2	7·1
	September.	5·5	4	5·7	9	9·2	11	8·6	5	24	169·1	7·0
	October.	7·0	1	6·7	8	6·6	8	6·8	6	23	157·7	6·9
	November.	7·2	2	7·5	11	7·1	7	8·1	7	28	210·7	7·5
	December.	7·1	5	6·2	6	6·1	5	8·2	1	18	111·3	6·2
Half-cloudy.	January.	......	......	5·8	7	6·0	6	4·4	5	18	96·8	5·4
	February.	3·2	1	3·6	7	8·4	5	3·7	6	19	106·7	5·6
	March.	5·1	5	4·2	11	7·3	3	4·4	9	28	132·9	4·7
	April.	5·6	6	4·8	8	5·3	8	3·4	3	25	125·4	5·0
	May.	5·1	6	6·1	5	5·1	4	5·9	6	21	117·3	5·6
	June.	5·3	6	5·8	7	5·5	9	5·0	7	21	115·4	5·5
	July.	4·7	6	4·9	6	6·6	7	6·3	5	24	134·9	5·6
	August.	5·8	6	5·2	11	6·5	7	7·5	5	29	174·6	6·0
	September.	4·7	16	4·2	9	7·2	7	12·0	2	34	197·6	5·8
	October.	4·1	11	6·1	4	5·4	6	6·0	6	26	137·9	5·3
	November.	6·1	6	6·0	5	4·6	1	7·6	6	24	153·0	6·4
	December.	5·8	9	5·1	3	7·9	1	......	0	13	75·3	5·8
Principally cloudy.	January.	......	......	3·8	2	4·6	6	2·6	7	15	53·6	3·6
	February.	3·9	2	3·1	6	4·9	3	3·1	6	17	59·7	3·5
	March.	3·2	5	1·6	1	10·5	1	4·2	5	12	48·8	4·1
	April.	2·2	6	3·9	1	2·8	1	7·3	2	10	34·6	3·5
	May.	2·1	8	5·0	5	5·8	6	6·0	5	20	74·7	3·7
	June.	3·0	5	5·8	1	5·5	9	−1·3	1	25	130·8	5·2
	July.	2·9	11	3·8	3	4·6	6	4·8	9	29	113·5	3·9
	August.	3·6	7	3·6	1	7·0	2	7·9	1	11	50·5	4·6
	September.	4·2	2	2·6	4	6·6	3	6·3	5	14	70·7	5·1
	October.	2·8	9	4·6	3	4·6	3	6·3	1	16	59·5	3·7
	November.	2·5	9	4·4	2	1·5	2	5·7	3	15	51·6	3·4
	December.	3·7	6	8·0	1	5·7	7	......	0	14	70·3	5·0
Cloudy.	January.	......	......	2·3	10	3·4	5	0·7	8	20	45·8	2·0
	February.	2·1	19	1·8	5	3·3	11	3·3	7	42	109·0	2·6
	March.	0·6	3	1·9	8	5·3	7	1·8	8	26	68·1	2·6
	April.	1·9	4	1·4	5	2·8	4	2·1	3	16	32·3	2·0
	May.	0·4	6	1·9	3	2·6	12	4·1	9	30	80·0	2·7
	June.	0·3	6	1·2	2	3·2	6	3·7	2	16	27·8	1·7
	July.	0·8	7	3·7	3	3·8	5	3·1	4	19	48·4	2·6
	August.	0·6	8	2·9	3	3·9	2	4·4	7	20	54·0	2·7
	September.	2·6	2	2·0	5	5·1	3	5·4	5	15	57·7	3·8
	October.	1·5	5	2·7	7	2·4	6	4·7	10	28	88·4	3·2
	November.	2·4	3	4·0	6	2·0	8	2·9	11	28	79·1	2·8
	December.	4·3	3	2·8	15	3·3	17	3·4	23	56	188·8	3·3

The numbers contained in this table show very clearly that, under the same state of the sky, the excess of the reading of the thermometer in air above that in the reflector is the same at all times of the year, and they as clearly show that the amount of the excess varies with every variation of the quantity of cloud. These results are exhibited in each year; and they are very decidedly shown in the last column of each division of the table, which contains the mean results for every month derived from the four years' observations, excepting in January, which was deduced from three years only.

Table C. is formed from the numbers in Table XCIX. by taking their yearly values for each state of the sky. The last column of this table contains the mean result from all the observations which were spread over 1419 nights; in the year 1841 there was one observation lost; in 1842 there were fifteen; in 1843 there were nine, and in 1844 there were six. These observations were lost chiefly from the instrument being out of repair, or the having omitted to set the index the previous day. The result in the last column is therefore deduced from 1389 nights, and include those from all states of the sky and weather.

Table C.—Showing the mean results in each year, with the different states of the sky, and for the whole time derived from all the self-registering observations of the thermometer whose bulb was placed in the focus of a metallic parabolic reflector.

State of the sky.	The mean yearly excess, according to the state of the sky with respect to the quantity of cloud, of the readings of a self-registering thermometer placed in air at the height of 4 feet and protected from radiation, above those of a similar thermometer placed in the focus of a metallic parabolic reflector fully exposed to the sky.																				
	1841.			1842.			1843.			1844.			1840 February 10, to 1844 December 31.								
	Whole sum of excesses.	Whole number of nights.	Mean excess.	Whole sum of excesses.	Whole number of nights.	Mean excess.	Whole sum of excesses.	Whole number of nights.	Mean excess.	Whole sum of excesses.	Whole number of nights.	Mean excess.	Whole number of nights.	Sum of excesses.	Mean excess.	Whole number of nights.	Sum of excesses.	Mean excess per night.			
Cloudy	97·3	66	1·47	177·8	72	2·47	281·4	86	3·27	322·9	97	3·33	321	879·4	2·74		°	°			
Principally cloudy...	203·9	70	2·91	118·8	30	3·96	258·2	49	5·27	237·4	49	4·85	198	818·3	4·13						
Half-cloudy..	396·0	78	5·08	430·5	82	5·25	394·9	62	6·37	336·4	60	5·60	282	1557·8	5·52	1389	7826·4	5·65			
Principally clear ...	270·8	43	6·30	578·4	81	7·14	591·1	78	7·58	374·4	55	6·81	257	1814·7	7·06						
Cloudless ...	535·9	66	8·12	682·1	85	8·03	768·1	81	9·48	770·1	99	7·78	331	2756·2	8·33						

By considering that the cloudy nights in this table are represented by 10; the principally cloudy by 8; the half-cloudy by 5; the principally clear by 2; and the cloudless by 0, the following particulars may be deduced from this table.

The sum of the products of the above numbers into the number of nights of each class in each year, divided by the number of nights in the year, gives the mean state of cloudiness during the nights of that year; and the yearly sum of the excess of the reading of the thermometer in air above that placed in the reflector, divided by the number of nights in the year, gives the excess, corresponding to the mean state of cloudiness; and thus we find

In the year 1841	The mean state of cloudiness during the nights was	. . $5^{\circ}\cdot8$,	and the mean excess of the reading of the thermometer in the air above that in the reflector was	. . $4^{\circ}\cdot7$
In the year 1842	„	. . 4·4,	„	. . 5·7
In the year 1843	„	. . 4·9,	„	. . 6·4
In the year 1844	„	. . 5·0,	„	. . 5·7

And the mean state of cloudiness during the nights of the four years was 5 (or the sky upon the average had been one-half covered by cloud), and the corresponding mean excess of the reading of the thermometer in air above that in the reflector was 5°·65.

In the year 1841 the number of clear and cloudy nights were equal; and they were nearly the same in number in the year 1844; in the year 1842 the number of clear nights exceeded the number of cloudy nights by thirteen; and in 1843 the cloudy exceeded the clear nights by five: from this it appears that during these four years there were one clear and one cloudy night out of every four nights.

In Table XLV. the relative less reading of a thermometer placed on raw wool and one in the focus of a parabolic reflector, than that in the air was found to be as 1221 : 888 from 889 simultaneous observations.

In Table CVI. this ratio is found to be as 1280 : 962 from 992 observations, deduced from self-registering minimum thermometers. As these results are so nearly identical, and as they have been deduced by entirely different instruments, there can be but little doubt of the correctness of their mean, viz. 1251 : 910, or in other words, the results as derived from the observations of the thermometer in the reflector, would be converted into results as deduced from placing a thermometer on raw wool by multiplying the former by 1·375, and in this way we should derive 7°·76 (*i. e.* 5°·65 × 1·375) as the mean deduced from the four years' observations; and we should find 11°·5 (*i. e.* 8°·33 × 1·375) as that deduced from cloudless nights. I have preferred exhibiting these results in terms of that derived from raw wool in consequence of it being free from the effects of heat conducted from the earth, and therefore free from one of the many sources from which the reading of a thermometer placed on the ground is affected, some of which are as follows:—

From the heat of the earth upon which it is placed.

From the heat radiated to it from lateral objects.

From the heat communicated to the substance from the air in contact with it.

From the heat evolved during the change of the watery vapour in the atmosphere into dew.

From the heat received from the radiation from clouds.

From the heat received from the upper regions of the atmosphere, and

From the heat received from space.

By placing a thermometer on wool the effect of the first of these causes is evaded, as appears in a previous section; in a wide and open plain the second would be evaded, as there would be but few objects to emit heat; I fear, however, that all my

readings are affected from this cause, and to an unknown amount; the heat of the air is known, and therefore it can be accounted for; the amount of heat evolved during the change of vapour into water appears to be about 4° from all the experiments I have made; the heat radiated from dense clouds, near the earth, must be very nearly the same in amount as that radiated from the earth, as the clouds when at a low elevation must possess very nearly the same heat as that of the lower atmosphere; but when such clouds are high their temperature must be less than when at a low elevation, and they will radiate less heat to the earth than they will receive from it (see the following experiments upon the different results deduced from high and low clouds, of the same modification and covering the same extent of sky); and the amount of heat from the other sources is unknown. The whole effect of all these checks upon the production of a great cold at night, by the radiation of heat from bodies on the surface of the earth, cannot be estimated, yet, notwithstanding their operation, the reading of a thermometer placed on the ground has been frequently very low. The reading of a thermometer thus placed represents the amount of heat received from all the above sources, diminished by the amount radiated from itself.

The place in which the observations were taken is not favourably situated for the production of a great cold, from radiation of heat at night, it being surrounded, at no great distance, by large trees, and consequently the humidity of the atmosphere is great.

From the circumstance of low readings always having taken place when the sky has been cloudless and bright, we may readily infer that the temperature of space must be very low indeed.

The reading of a thermometer placed on grass is much affected by the heat conducted to it from the earth beneath; yet, notwithstanding, its readings were always less than those of the thermometer in the reflector, in the ratio of 1000 : 858 (see Table XLV.), therefore it is necessary to multiply the results derived from the latter by 1·17 to reduce them to results that would have been derived from the former.

By examining the numbers in the columns under the lowest thermometrical readings in Table CVII., it appears that long grass, and therefore vegetation is liable to be affected at night from the influence of radiation by a temperature below the freezing-point of water every month in the year, for even in July 1844, the only exception in that year, the thermometer read 35°·5, whilst that in the reflector read 39°·3; in the year 1843, in July, the reading in the reflector was as low as 35°·2, and it seems very probable that long grass temperature at this time was at or below 32°; and as all the readings would have been lower if the experiments had been made in the open country, it seems certain that vegetation is always liable to the temperature of 32° in this country.

The next table is formed by taking the means of all the numbers contained in each division of the Tables LXXVIII. to XCVIII.

TABLE CI.—Showing the mean monthly excess of the reading of a self-registering minimum thermometer in Air at the height of 4 feet, above the

General state of the sky during the night previous to reading the instruments.	Year.	Month.	The mean excess in each month, according to the state of the sky with respect to the quantity of cloud, of the readings of a self-registering minimum thermometer placed in air at the height of 4 feet and protected											
			On long grass exposed to ¾ths of the sky.		On long grass.		On short grass.		On garden mould.		In air one inch high.		In air three inches high.	
			Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.
Cloudless.	1843.	April.	° 8·1	1	° ...	...	° 8·5	1	° 5·6	3	° 8·6	1	° 7·9	1
		May.	6·0	2	...	...	10·2	2	4·0	2	11·4	2	7·7	2
		June.	6·5	7	...	...	9·7	7	5·5	7	10·5	7	11·3	7
		July.	7·4	2	...	...	12·7	2	10·0	1	12·0	2	10·0	2
		August.	5·8	6	...	...	10·6	6	...	...	10·8	6	10·9	6
		September.	7·1	4	11·4	7	10·0	11	...	...	8·8	11	6·9	11
		October.	...	...	8·0	8	6·4	8	...	...	5·3	8	...	...
		November.	...	...	9·9	6	8·0	6	...	...	6·3	1	...	...
		December.	...	...	...	0	...	0	...	...	...	0	...	...
	1844.	January.	...	...	9·5	2	9·9	1	...	...	...	...	...	...
		February.	...	...	9·6	4	7·1	4	...	...	...	...	...	...
		March.	...	...	8·3	7	6·2	7	...	...	...	...	...	...
		April.	...	...	11·8	18	10·7	18	...	...	...	...	...	...
		May.	...	...	11·3	10	10·6	10	...	...	...	...	...	...
		June.	...	...	9·3	7	8·0	7	...	...	...	...	...	...
		July.	...	...	10·9	12	9·1	12	...	...	...	...	...	...
		August.	...	...	8·9	12	7·6	12	...	...	...	...	...	...
		September.	...	...	10·5	12	8·7	12	...	...	...	...	...	...
		October.	...	...	10·7	8	8·7	8	...	...	...	...	...	...
		November.	...	...	10·1	2	5·4	2	...	...	...	...	...	...
		December.	...	...	11·0	6	7·2	6	...	...	...	...	...	...
Principally clear.	1843.	April.	...	...	...	...	11·0	2	2·3	2	...	...	...	...
		May.	4·5	7	...	...	5·0	7	3·3	7	7·6	7	6·7	7
		June.	3·5	7	...	...	6·0	7	2·9	7	8·3	7	7·9	7
		July.	5·9	10	...	...	5·9	10	7·0	1	7·1	9	7·3	10
		August.	4·7	12	...	...	7·1	12	...	...	6·9	12	7·2	12
		September.	7·4	1	8·8	2	9·7	5	...	...	9·1	5	9·5	5
		October.	...	...	8·0	8	7·1	8	...	...	5·9	8	...	...
		November.	...	...	8·9	7	6·2	6	...	...	4·4	4	...	...
		December.	...	...	7·1	5	6·5	4	...	...	...	...	...	...
	1844.	January.	...	...	8·5	7	7·4	7	...	...	...	...	...	...
		February.	...	...	7·5	6	5·8	6	...	...	...	...	...	...
		March.	...	...	6·6	2	5·6	2	...	...	...	...	...	...
		April.	...	...	10·3	4	9·1	4	...	...	...	...	...	...
		May.	...	...	9·8	5	9·5	5	...	...	...	...	...	...
		June.	...	...	8·8	5	9·0	5	...	...	...	...	...	...
		July.	...	...	8·8	1	10·0	1	...	...	...	...	...	...
		August.	...	...	8·1	6	6·0	6	...	...	...	...	...	...
		September.	...	...	10·8	4	9·9	4	...	...	...	...	...	...
		October.	...	...	9·5	6	8·1	6	...	...	...	...	...	...
		November.	...	...	9·6	7	7·2	7	...	...	...	...	...	...
		December.	...	...	7·2	1	5·7	1	...	...	...	...	...	...
Half-cloudy.	1843.	April.	4·7	1	...	...	6·2	3	3·3	3	4·7	1	4·2	1
		May.	4·1	4	...	...	6·6	4	4·2	4	7·1	4	7·6	4
		June.	3·8	1	...	...	7·3	1	4·0	1	10·8	1	10·6	1
		July.	3·8	7	...	...	6·2	7	4·5	1	5·9	7	6·4	7
		August.	4·5	7	...	...	6·0	7	...	...	5·6	7	6·0	7
		September.	4·7	2	9·4	5	8·1	7	...	...	6·5	7	4·8	7
		October.	...	...	6·3	6	5·6	6	...	...	4·8	6	3·3	1
		November.	...	...	4·5	7	5·6	6	...	...	3·8	1	...	...
		December.	...	...	7·1	1	6·1	1	...	...	...	...	...	...
	1844.	January.	...	...	7·4	5	5·9	5	...	...	...	...	...	...
		February.	...	...	6·2	6	4·6	6	...	...	...	...	...	...
		March.	...	...	6·5	9	4·7	9	...	...	...	...	...	...
		April.	...	...	6·2	3	6·0	3	...	...	...	...	...	...
		May.	...	...	5·5	9	5·4	9	...	...	...	...	...	...
		June.	...	...	7·7	7	7·6	7	...	...	...	...	...	...
		July.	...	...	9·7	5	6·8	4	...	...	...	...	...	...
		August.	...	...	8·4	4	6·4	4	...	...	...	...	...	...
		September.	...	...	10·5	12	8·7	12	...	...	...	...	...	...
		October.	...	...	7·6	6	6·7	6	...	...	...	...	...	...
		November.	...	...	8·0	6	6·4	6	...	...	...	...	...	...
		December.	...	...	...	0	...	0	...	...	...	...	...	...

readings of a similar thermometer placed on different substances, in each state of the sky.

from the effects of radiation, above that of a similar thermometer fully exposed to the sky, placed																	
In air six inches high.		On white raw wool.		On fine flax.		On coarse flax.		On white unwrought cotton wool.		On lead.		On blackened tin.		On white tin.		In focus of metallic parabolic reflector.	
Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.
°		°		°		°		°		°		°		°		°	
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	10·1	11
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	10 5	2
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	10·3	7
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	10·1	2
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	9·1	6
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	9·2	11
...	...	11·7	7	11·0	7	10·0	7	9·4	7	...	...	...	...	...	...	6·6	8
...	...	12·3	5	11·5	6	12·3	5	11·9	4	...	...	...	...	...	...	8·5	6
...	...	...	0	...	0	...	...	...	...	...	...	...	0	...	0	...	0
...	...	13·4	1	10·6	1	...	...	...	...	...	...	9·6	1	8·9	1	8·0	2
...	...	8·8	4	9·1	4	...	...	...	...	...	...	7·6	4	7·7	4	7·0	4
...	...	11·2	7	9·8	7	...	...	...	...	5·9	7	...	...	...	...	6·5	7
...	...	15·3	18	16·2	18	...	...	...	...	9·2	18	...	...	...	...	8·0	18
...	...	11·9	10	13·2	10	...	...	...	...	9·6	4	...	...	...	...	8·1	10
...	...	8·3	4	7·8	4	...	...	...	...	...	...	...	...	...	...	6·8	6
...	...	12·3	9	13·1	9	...	...	...	...	...	...	...	...	...	...	8·2	12
...	...	8·6	12	9·8	12	...	...	...	...	...	...	...	...	...	...	7·1	12
...	...	...	...	12·6	12	...	...	...	...	...	...	...	...	...	...	9·7	12
...	...	...	...	12·4	8	...	...	...	...	...	...	...	...	...	...	6·8	8
...	...	...	...	9·7	2	...	...	...	...	...	...	...	...	...	...	6·0	2
...	...	...	...	12·3	6	...	...	...	...	...	...	...	...	...	...	9·3	6
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7·4	6
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7·8	4
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7·6	6
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	8·4	10
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	8·1	12
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	9·2	11
3·1	8·0	8·8	2	9·3	2	8·9	2	8·6	2	...	...	...	...	...	...	6·6	8
...	...	9·5	7	9·1	7	9·5	5	8·0	5	...	...	...	...	...	...	7·1	7
...	...	7·6	5	7·0	5	...	...	...	...	...	...	5·9	5	4·9	5	6·1	5
...	...	10·8	7	8·6	7	...	...	...	...	...	...	8·0	7	7·3	7	4·6	7
...	...	7·9	6	7·9	6	...	...	...	...	...	...	7·0	6	6·8	6	5·5	6
...	...	7·5	2	7·8	2	...	...	...	...	5·2	2	...	...	...	...	4·9	2
...	...	13·1	4	14·5	4	...	...	...	...	10·1	4	...	...	...	...	7·4	4
...	...	11·4	4	9·1	4	...	...	...	...	...	...	...	...	...	...	6 0	5
...	...	10·3	4	10·4	5	...	...	...	...	...	...	...	...	...	...	7·7	5
...	...	12·9	1	13·1	1	...	...	...	...	...	...	...	...	...	...	7·1	1
...	...	8·1	6	8·3	6	...	...	...	...	...	...	...	...	...	...	7·5	6
...	...	...	...	12·5	4	...	...	...	...	...	...	...	...	...	...	8·6	5
...	...	...	...	10·4	6	...	...	...	...	...	...	...	...	...	...	8·8	6
...	...	...	...	10·1	7	...	...	...	...	...	...	...	...	...	...	8·1	7
...	...	...	...	7·1	1	...	...	...	...	...	...	...	...	...	...	8·2	1
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5·3	8
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5·1	4
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5·5	9
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6·6	7
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6·5	7
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7·2	7
4·2	3·0	5·4	2	4·6	2	4·8	2	4·0	2	5·0	...	...	...	...	...	5·4	6
...	...	6·5	6	8·0	4	8·6	1	4·8	2	...	...	...	...	...	...	4·6	1
...	...	6·9	1	...	...	7·3	1	...	...	...	...	5·9	1	5·9	1	7·9	1
...	...	9·1	5	5·4	5	...	...	...	...	...	...	6·8	5	5·4	5	4·4	5
...	...	4·2	6	5·0	6	...	...	...	...	...	...	5·1	6	4·5	6	3·7	6
...	...	6·7	8	6·9	8	...	...	...	...	5·3	8	...	...	...	...	4·4	9
...	...	8·2	3	8·8	3	...	...	...	...	4·6	3	...	...	...	...	3·4	3
...	...	8·7	4	8·4	4	...	...	...	...	...	...	...	...	...	...	5·9	6
...	...	7·9	2	6·6	4	...	...	...	...	...	...	...	...	...	...	5·0	7
...	...	8·1	3	8·3	3	...	...	...	...	...	...	...	...	...	...	6·3	5
...	...	11·0	4	11·9	4	...	...	...	...	...	...	...	...	...	...	7·5	5
...	...	...	...	12·6	10	...	...	...	...	...	...	...	...	...	...	12·0	2
...	...	...	...	9·3	6	...	...	...	...	...	...	...	...	...	...	6·0	6
...	...	...	...	8·9	6	...	...	...	...	...	...	...	...	...	...	7·6	6
...	...	...	...	...	0	...	...	...	...	...	...	...	...	...	...	...	0

Table (Continued).

General state of the sky during the night previous to reading the instruments.	Year.	Month.	The mean excess in each month, according to the state of the sky with respect to the quantity of clouds, of the readings of a self-registering minimum thermometer placed in air at the height of 4 feet and protected											
			On long grass exposed to $\frac{3}{4}$ths of the sky.		On long grass.		On short grass.		On garden mould.		In air one inch high.		In air three inches high.	
			Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.
			°		°		°		°		°		°	
Principally cloudy.	1843.	April.	...	...	...	...	...	...	...	...	...	...	...	...
		May.	3·8	6	...	...	3·6	6	3·1	6	5·8	6	5·6	6
		June.	4·5	9	...	...	4·8	9	4·4	9	5·2	9	6·0	9
		July.	3·3	6	...	...	3·1	6	...	...	4·3	5	4·0	6
		August.	5·1	2	...	...	7·0	2	...	...	6·8	2	7·3	2
		September.	5·4	1	6·2	1	5·0	3	...	...	4·0	3	3·6	3
		October.	...	...	6·6	3	4·0	3	...	...	3·4	3	3·3	2
		November.	...	...	3·8	2	4·0	2	...	...	...	...	...	...
		December.	...	...	6·1	7	5·7	7	...	...	...	...	...	...
	1844.	January.	...	...	6·9	7	6·2	7	...	...	...	...	...	...
		February.	...	...	5·6	6	3·4	6	...	...	...	...	...	...
		March.	...	...	6·6	5	5·1	5	...	...	...	...	...	...
		April.	...	...	9·5	2	8·9	2	...	...	...	...	...	...
		May.	...	...	5·2	1	4·7	1	...	...	...	...	...	...
		June.	...	...	8·7	10	7·9	10	...	...	...	...	...	...
		July.	...	...	7·5	9	7·2	9	...	...	...	...	...	...
		August.	...	...	4·9	7	4·4	7	...	...	...	...	...	...
		September.	...	...	6·6	5	5·2	5	...	...	...	...	...	...
		October.	...	...	5·6	1	5·8	1	...	...	...	...	...	...
		November.	...	...	6·0	3	3·8	3	...	...	...	...	...	...
		December.	...	...	...	0	...	0	...	...	...	...	...	...
Cloudy.	1843.	April.	5·7	2	...	...	6·8	2	4·2	2	5·4	2	5·2	2
		May.	3·7	12	...	...	2·8	12	2·1	12	3·7	12	4·8	12
		June.	2·3	5	...	...	1·5	5	1·2	5	2·9	5	3·1	5
		July.	3·0	5	...	...	2·1	5	...	...	2·9	4	2·3	5
		August.	1·5	2	...	...	1·2	2	...	...	1·5	2	1·7	2
		September.	3·0	3	...	...	2·9	3	...	...	3·1	3	3·1	3
		October.	3·7	5	3·7	5	4·2	5	...	...	1·1	5	...	...
		November.	...	...	2·1	8	1·7	7	...	...	1·9	2	...	...
		December.	...	...	3·0	17	2·8	17	...	...	...	...	...	...
	1844.	January.	...	...	3·5	8	2·9	7	...	...	...	...	...	...
		February.	...	...	3·9	7	3·6	7	...	...	...	...	...	...
		March.	...	...	3·2	8	2·8	8	...	...	...	...	...	...
		April.	...	...	2·6	3	2·6	3	...	...	...	...	...	...
		May.	...	...	5·5	9	5·4	9	...	...	...	...	...	...
		June.	...	...	1·7	2	1·6	2	...	...	...	...	...	...
		July.	...	...	3·2	4	2·2	4	...	...	...	...	...	...
		August.	...	...	4·9	7	4·4	7	...	...	...	...	...	...
		September.	...	...	3·1	6	2·9	6	...	...	...	...	...	...
		October.	...	...	5·3	10	4·2	10	...	...	...	...	...	...
		November.	...	...	3·2	12	2·5	12	...	...	...	...	...	...
		December.	...	...	2·7	23	1·1	23	...	...	...	...	...	...

TABLE (Continued).

from the effects of radiation, above that of a similar thermometer fully exposed to the sky, placed																	
In air six inches high.		On white raw wool.		On fine flax.		On coarse flax.		On white unwrought cotton wool.		On lead.		On blackened tin.		On white tin.		In focus of metallic parabolic reflector.	
Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.	Mean excess.	Number of nights.
°		°		°		°		°		°		°		°		°	
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2·8	1
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5·8	6
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5·5	9
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4·6	6
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7·0	2
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6·6	3
...	...	2·5	1	2·5	1	2·5	1	...	...	...	...	...	...	...	...	4·6	3
...	...	4·5	2	4·3	2	...	...	...	...	...	...	...	...	...	...	1·5	2
...	...	6·0	7	...	...	6·1	7	...	...	...	...	5·1	7	4·0	6	5·7	7
...	...	7·0	7	5·6	7	...	...	...	...	...	...	6·7	7	6·3	7	2·6	7
...	...	7·4	6	6·8	6	...	...	...	...	...	...	4·9	6	4·6	6	3·1	6
...	...	8·0	5	7·3	5	...	...	...	...	5·3	1	...	...	...	...	4·2	5
...	...	12·3	2	13·2	2	...	...	...	...	9·0	2	...	...	...	...	7·3	2
...	...	8·7	1	8·7	1	...	...	...	...	...	...	...	...	...	...	6·0	5
...	...	9·1	8	9·0	8	...	...	...	...	...	...	...	...	...	...	−1·3	1
...	...	8·5	7	7·7	7	...	...	...	...	...	...	...	...	...	...	4·8	9
...	...	6·5	5	6 0	6	...	...	...	...	...	...	...	...	...	...	7·9	1
...	...	...	...	7·6	5	...	...	...	...	...	...	...	...	...	...	6·3	5
...	...	...	...	8·8	1	...	...	...	...	...	...	...	...	...	...	6·3	1
...	...	...	...	6·6	3	...	...	...	...	...	...	...	...	...	...	5·7	3
...	...	...	...	...	0	...	...	...	...	...	...	...	...	...	...	...	0
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2·8	4
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2·6	12
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3·2	6
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3·8	5
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3·9	2
2·0	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5·1	3
...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2·4	6
...	...	3·3	7	2·0	5	2·9	4	2·1	4	...	...	2·2	15	2·4	16	2·0	8
...	...	2·4	17	...	...	3·0	17	...	...	...	...	3·5	7	3·7	7	3·3	17
...	...	3·6	8	2·5	8	...	...	...	...	...	...	5·5	6	4·6	6	0·7	8
...	...	4·7	6	5·1	6	...	...	...	...	...	...	...	...	...	...	3·3	7
...	...	3·5	8	3·8	8	...	...	...	...	2·9	8	...	...	...	...	1·8	8
...	...	5·2	3	4·9	3	...	...	...	...	2·6	3	...	...	...	...	2·1	3
...	...	8·7	4	8·4	4	...	...	...	...	...	...	...	...	...	...	4·1	9
...	...	1·8	2	2·5	2	...	...	...	...	...	...	...	...	...	...	3·7	2
...	...	3·3	3	3·6	3	...	...	...	...	...	...	...	...	...	...	3·1	4
...	...	6·5	5	6·0	6	...	...	...	...	...	...	...	...	...	...	4·4	7
...	...	...	...	3·5	6	...	...	...	...	...	...	...	...	...	...	5·4	5
...	...	...	...	7·1	10	...	...	...	...	...	...	...	...	...	...	4·7	10
...	...	...	...	2·5	12	...	...	...	...	...	...	...	...	...	...	2·9	11
...	...	...	...	2·4	23	...	...	...	...	...	...	...	...	...	...	3·4	23

The following Table contains the mean of the values in each year, and for the whole time according to the number of observations.

Table CII.—Showing the excess of reading of the thermometer in Air, above those placed as stated in the 2nd column, in each of the years 1843 and 1844, and for both years together.

General state of the sky during the night previous to reading the instruments.	Situation of the thermometer.	The mean yearly excess, according to the state of the sky, of the reading of a self-registering minimum thermometer placed in air at the height of 4 feet, and protected from radiation, above that of a similar thermometer placed on different substances.								
		1843.			1844.			1843 and 1844.		
		Number of nights.	Sum of excesses.	Mean excess.	Number of nights.	Sum of excesses.	Mean excess.	Whole number of nights.	Whole sum of excesses.	Mean excess.
Cloudy.	On long grass exposed to $\frac{3}{4}$ths of the sky	29	95·6	3·3	...	......	...	29	95·6	3·3
	On long grass fully exposed to the sky	30	85·9	2·9	99	360·2	3·7	129	446·1	3·4
	On short grass fully exposed to the sky	58	159·5	2·8	98	281·6	2·9	156	441·1	2·8
	On garden mould	29	40·2	1·4	...	......	...	29	40·2	1·4
	One inch above grass	34	103·0	3·0	...	......	...	34	103·0	3·0
	Three inches above grass	31	111·5	3·6	...	......	...	31	111·5	3·6
	Six inches above grass	...	......	...	...	......	...	...	......	...
	On white raw wool	25	70·6	2·8	40	186·5	4·7	65	257·1	3·9
	On fine flax	6	15·7	2·6	92	363·8	4·0	98	379·5	3·9
	On coarse flax	22	67·6	3·1	...	......	...	22	67·6	3·1
	On white unwrought cotton wool	5	11·2	2·2	...	......	...	5	11·2	2·2
	On lead	...	......	...	11	31·0	2·8	11	31·0	2·8
	On blackened tin	15	33·5	2·2	14	62·7	4·5	29	96·2	3·3
	On white tin	16	38·4	2·4	14	50·7	3·6	40	89·1	2·2
	In focus of metallic reflector	86	281·4	3·3	97	322·9	3·3	183	604·3	3·3
Principally cloudy.	On long grass exposed to $\frac{3}{4}$ths of the sky	25	101·2	4·1	...	......	...	25	101·2	4·0
	On long grass fully exposed to the sky	13	76·9	5·9	50	354·7	7·1	63	431·6	6·8
	On short grass fully exposed	39	181·8	4·6	50	303·3	6·1	89	485·1	5·5
	On garden mould fully exposed	16	59·7	3·7	...	......	...	16	59·7	3·7
	One inch above grass	28	146·0	5·2	...	......	...	28	146·0	5·2
	Three inches above grass	29	149·0	5·1	...	......	...	29	149·0	5·2
	Six inches above grass	...	......	...	...	......	...	...	......	...
	On white raw wool	10	53·9	5·4	37	302·0	8·2	47	355·9	7·5
	On fine flax	3	11·1	3·7	46	352·9	7·7	49	364·0	7·4
	On coarse flax	8	45·4	5·7	...	......	...	8	45·4	5·7
	On white unwrought cotton wool	1	6·5	6·5	...	......	...	1	6·5	6·5
	On lead	...	......	...	7	44·3	6·3	7	44·3	6·3
	On blackened tin	7	36·1	5·1	13	75·8	5·8	20	111·9	5·6
	On white tin	6	24·2	4·0	13	71·7	5·3	19	95·9	5·0
	In focus of metallic reflector	49	258·2	5·3	49	237·4	4·9	98	495·6	5·1
Half-cloudy.	On long grass exposed to $\frac{3}{4}$ths of the sky	22	88·6	4·0	...	......	...	22	88·6	4·0
	On long grass fully exposed	19	120·7	6·4	58	438·4	7·6	77	559·1	7·3
	On short grass	42	267·3	6·4	57	353·6	6·2	99	620·9	6·3
	On garden mould	10	39·7	4·0	...	......	...	10	39·7	3·9
	One inch above grass	34	202·5	5·9	...	......	...	34	202·5	5·9
	Three inches above grass	28	168·9	6·0	...	......	...	28	168·9	6·0
	Six inches above grass	3	12·5	4·2	...	......	...	3	12·5	4·2
	On white raw wool	9	56·6	6·3	33	250·7	7·6	42	307·3	7·3
	On fine flax	11	41·2	3·7	49	383·9	7·8	60	425·1	7·1
	On coarse flax	4	25·5	6·4	...	......	...	4	25·5	6·4
	On white unwrought cotton wool	4	17·6	4·4	...	......	...	4	17·6	4·4
	On lead	...	......	...	14	70·7	5·1	14	70·7	5·1
	On blackened tin	1	5·9	5·9	11	64·5	5·9	12	70·4	5·9
	On white tin	1	5·9	5·9	11	53·9	4·9	12	59·8	5·0
	In focus of metallic reflector	62	394·9	6·4	60	336·4	5·6	122	731·3	6·0

TABLE (Continued).

General state of the sky during the night previous to reading the instruments.	Situation of the thermometer.	The mean yearly excess, according to the state of the sky, of the reading of a self-registering minimum thermometer placed in air at the height of 4 feet, and protected from radiation, above that of a similar thermometer placed on different substances.								
		1843.			1844.			1843 and 1844.		
		Number of nights.	Sum of excesses.	Mean excess.	Number of nights.	Sum of excesses.	Mean excess.	Whole number of nights.	Whole sum of excesses.	Mean excess.
Principally clear.	On long grass exposed to $\frac{3}{4}$ths of the sky	37	178·5	4·8	...	......	...	37	178·5	4·8
	On long grass fully exposed to the sky	22	179·2	8·1	55	195·2	9·0	77	374·4	4·9
	On short grass	61	410·7	6·7	55	427·7	7·8	116	838·4	7·2
	On garden mould	17	55·6	3·3	...	......	...	17	55·6	3·3
	One inch above grass	52	368·3	7·1	...	......	...	52	368·3	7·1
	Three inches above grass	41	295·1	7·1	...	......	...	41	295·1	7·2
	Six inches above grass	6	18·7	3·2	...	......	...	6	18·7	3·1
	On white raw wool	14	122·6	8·8	35	299·7	8·6	49	422·3	3·6
	On fine flax	9	82·6	9·2	54	482·2	8·9	63	564·8	9·0
	On coarse flax	12	100·2	8·3	...	......	...	12	100·2	8·4
	On white unwrought cotton wool	7	57·2	8·2	...	......	...	7	57·2	8·2
	On lead	...	......	...	11	61·1	5·5	11	61·1	5·6
	On blackened tin	5	29·6	5·9	13	97·8	7·5	18	127·4	7·1
	On white tin	5	24·3	4·9	13	91·9	7·1	18	116·2	6·4
	In focus of metallic reflector	78	591·1	7·6	55	374·4	6·8	133	965·5	7·3
Cloudless.	On long grass exposed to $\frac{3}{4}$ths of the sky	22	143·6	6·5	...	......	...	22	143·6	6·5
	On long grass fully exposed to the sky	21	203·0	9·7	100	920·2	9·2	121	1123·2	9·3
	On short grass	47	322·1	6·9	98	866·1	8·9	145	1188·2	8·2
	On garden mould	13	73·8	5·7	...	......	...	13	73·8	5·7
	One inch above grass	38	341·2	5·7	...	......	...	38	341·2	9·0
	Three inches above grass	29	214·0	7·4	...	......	...	29	214·0	7·4
	Six inches above grass	1	5·2	5·2	...	......	...	1	5·2	5·2
	On white raw wool	13	153·5	11·8	60	726·2	12·1	73	879·7	12·1
	On fine flax	17	146·6	8·6	91	1123·6	12·3	108	1270·2	11·8
	On coarse flax	12	131·7	11·0	...	......	...	12	131·7	10·9
	On white unwrought cotton wool	11	113·1	10·3	...	......	...	11	113·1	10·3
	On lead	...	......	...	29	245·8	8·5	29	245·8	8·5
	On blackened tin	...	......	...	5	40·1	8·0	5	40·1	8·0
	On white tin	...	......	...	5	39·8	8·0	5	39·8	7·9
	In focus of metallic reflector	81	768·1	9·5	99	770·1	7·8	180	1538·2	8·5

An examination of this Table shows, that on cloudy nights the results derived from every substance, with the single exception of garden mould, are nearly the same; and on partially cloudy nights they are different; the greatest differences occurring on cloudless nights. The reading of the thermometer on garden mould was always the nearest to the reading of the thermometer in the air; those on raw wool and on flax departed the most from it; and those placed, at the distance of one inch to two inches above the top of grass, on a sheet of lead or tin, were all higher than that on garden mould and less than that on wool; and they were all nearly identical with that of the thermometer in the reflector, notwithstanding much heat during the course of a whole night must have passed from the earth to the surface of the lead and tin, which were placed on it.

In this respect the results are somewhat different from those obtained from simultaneous observations of mercurial thermometers; but it would certainly seem that a thermometer placed on a sheet of metal on the ground will read as low as that in the reflector, although that of the latter would attain its lowest reading in less time than that of the former.

From the numbers in this table the numbers in the first ten columns of Table CVI. are formed.

As in a previous part of this paper it has been stated that on cloudy nights a thermometer read differently according as the cloud was high or low, the following Table has been formed by collecting all those cases in which the night has been cloudy throughout.

Table CIII.—Showing the results on totally cloudy nights, but the clouds *not* cirrostratus.

The sky covered with cloud during the night, the cloud other than cirrostratus.											
Year.	Month, day and hour of reading the instruments.	The excess of the reading of a self-registering minimum therm. placed in air at the height of 4 feet and protected from radiation, above that of a similar thermometer fully exposed to the sky, placed									
		On long grass.	On short grass.	On white raw wool.	On fine flax.	On coarse flax.	On white unwrought cotton wool.	On blackened tin.	On white tin.	In focus of metallic parabolic reflector.	
	d h	°	°	°	°	°	°	°	°	°	°
1842.	Nov. 7 21	......	......	......	......	......	......	......	......	9·3	......
	9 21	......	......	......	......	......	......	......	......	9·0	......
	17 21	......	......	......	......	......	......	......	......	9·4	......
	20 21	......	......	......	......	......	......	......	......	8·7	......
1843.	Feb. 14 21	......	......	......	......	......	......	......	......	13·3	......
	25 21	......	......	......	......	......	......	......	......	8·5	......
	Nov. 10 21	9·7	7·6	12·6	12·3	13·4	12·2	......	......	9·5	32·6
1844.	Feb. 14 21	8·5	7·6	14·8	13·8	......	......	7·3	6·8	7·5	36·8
	May 9 21	10·7	9·9	9·3	9·4	......	......	......	......	11·4	51·7
	Oct. 10 21	10·0	6·4	......	11·5	......	......	......	......	7·9	44·5

Table CIV. has been formed by collecting all those cases in which the night has been cloudy throughout, and the clouds have been noted as being high; and Table CV. has been formed in a similar way from all those marked low. Those cloudy nights during which the height of the cloud was not noted have not been used. The means of the results in Tables CIII., CIV. and CV. are contained in Table CVI.

TABLE CIV.—Showing the results, at times with a totally cloudy sky, the clouds being cirrostratus from 3000 to 4000 feet high.

Year.	Month, day and hour of reading the instruments.	The sky covered with a high cirrostratus cloud during the night previous to reading the instruments. The excess of the reading of a self-registering minimum thermometer, placed in air at the height of 4 feet and protected from radiation, above that of a similar thermometer fully exposed to the sky, placed: On long grass exposed to ⅞ths of the sky.	On long grass.	On short grass.	On garden mould.	In air one inch high.	In air three inches high.	In air six inches high.	On white raw wool.	On fine flax.	On coarse flax.	On white unwrought cotton wool.	On lead.	On blackened tin.	On white tin.	In focus of metallic parabolic reflector.	Minimum reading of a therm. at the height of 4 feet protected from radiation.
	d h	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
1843.	April 28. 21	4·0	......	5·6	2·3	4·8	3·0	......	......	......	......	......	......	......	......	6·6	41·8
	29. 21	7·3	......	8·1	6·2	6·0	7·3	......	......	......	......	......	......	......	......	7·3	46·0
	May 12. 21	7·5	......	5·5	6·0	3·3	7·6	......	......	......	......	......	......	......	......	2·1	51·3
	13. 21	3·0	......	4·5	−0·5	4·5	3·5	......	......	......	......	......	......	......	......	3·8	45·0
	15. 21	5·4	......	2·1	2·4	3·4	3·2	......	......	......	......	......	......	......	......	3·7	49·4
	16. 21	9·3	......	6·3	......	7·3	10·3	......	......	......	......	......	......	......	......	5·0	49·3
	23. 21	4·6	......	6·1	5·8	8·9	9·0	......	......	......	......	......	......	......	......	2·8	52·6
	June 14. 21	4·5	......	5·9	3·6	6·8	7·1	......	......	......	......	......	......	......	......	6·4	57·0
	July 18. 21	3·8	......	9·8	......	9·6	5·1	......	......	......	......	......	......	......	......	7·3	52·3
	20. 21	4·0	......	4·0	......	4·0	4·1	......	......	......	......	......	......	......	......	5·1	54·1
	September 3. 21	5·7	......	3·7	......	4·2	4·6	......	......	......	......	......	......	......	......	7·2	57·2
	8. 21	4·8	......	5·4	......	5·3	5·6	......	......	......	......	......	......	......	......	8·6	61·6
	October 11. 21	......	7·3	5·0	......	3·8	......	2·3	......	......	......	......	......	......	......	6·4	42·3
	November 5. 21	......	6·3	4·0	......	3·5	......	......	7·7	6·5	8·5	4·7	......	......	......	3·7	40·7
	Decemb. 13. 21	......	6·8	7·8	......	......	......	......	8·3	......	7·1	......	......	6·5	5·3	2·3	39·3
	16. 21	......	7·5	5·9	......	......	......	......	5·6	......	5·9	......	......	......	3·2	5·7	44·7
	20. 21	......	3·9	2·4	......	......	......	......	1·9	......	2·2	......	......	1·9	1·9	3·2	41·4
	23. 21	......	5·6	4·8	......	......	......	......	4·2	......	5·0	......	......	4·0	3·8	4·5	49·8
	29. 21	......	3·2	3·2	......	......	......	......	3·3	......	3·5	......	......	2·5	2 6	5·2	38·5
	30. 21	......	2·1	2·5	......	......	......	......	2·1	......	2·8	......	......	2·1	2·5	3·7	39·3
1844.	January 8. 21																
	17. 21	......	4·5	4·2	......	......	......	......	3·9	3·3	......	......	......	4·5	1·7	0·5	36·5
	23. 21	......	4·6	4·2	......	......	......	......	5·9	4·6	......	......	......	4·9	3·9	2·4	36·2
	February 11. 21	......	6·0	4·1	......	......	......	......	7·2	4·1	......	......	......	5·2	4·7	4·7	30·2
	March 4. 21	......	5·9	4·2	......	......	......	......	5·6	5·2	......	......	5·3	......	......	5·9	32·4
	8. 21	......	4·6	3 0	......	......	......	......	3·8	3·1	......	......	3·0	......	......	3·5	36·2
	16. 21	......	8·1	7·7	......	......	......	......	9·1	9·1	......	......	6·3	......	......	4·1	46·1
	April 13. 21	......	4·9	5·1	......	......	......	......	7·7	8·4	......	......	5·3	......	......	3·1	48·7
	August 24. 21	......	7·6	5·8	......	......	......	......	8·5	7·3	......	......	......	......	......	6·5	52·3
	25. 21	......	4·4	3·2	......	......	......	......	......	5·3	......	......	......	......	......	9·1	52·4
	Septemb. 14. 21	......	4·8	4·4	......	......	......	......	......	4·9	......	......	......	......	......	6·0	57·3
	15. 21	......	4·1	4 6	......	......	......	......	......	4·8	......	......	......	......	......	5·9	61·4
	October 1. 21	......	5·1	4·6	......	......	......	......	......	5·6	......	......	......	......	......	3·2	57·6
	12. 21	......	5·7	6·0	......	......	......	......	......	9·8	......	......	......	......	......	3·6	55·8
	20. 21	......	10·0	6·4	......	......	......	......	......	11·5	......	......	......	......	......	7·9	44·5
	23. 21	......	7·7	9·7	......	......	......	......	......	13·3	......	......	......	......	......	5·0	35·5
	25. 21	......	9·1	5·5	......	......	......	......	......	16·1	......	......	......	......	......	7·9	43·1
	November 5. 21	......	5·1	2·6	......	......	......	......	......	4·1	......	......	......	......	......	2·2	39·1
	14. 21	......	5·2	5·2	......	......	......	......	......	1·8	......	......	......	......	......	2·8	47·6
	Decemb. 15. 21	......	5·1	3·6	......	......	......	......	......	4·6	......	......	......	......	......	4·8	35·4
	22. 21	......	4·9	2·9	......	......	......	......	......	4·1	......	......	......	......	......	5·9	29·1
	30. 21	......	5·8	3·4	......	......	......	......	......	2·5	......	......	......	......	......	5·9	34·1
	31. 21	......	4·9	2·7	......	......	......	......	......	2·9	......	......	......	......	......	3·9	35·1

TABLE CV.—Showing the results with a totally cloudy sky, the clouds being cirrostratus and less than 2000 feet high.

The sky covered with a low cirrostratus cloud during the night previous to reading the instruments.																
Year.	Month, day and hour of reading the instruments.	The excess of the reading of a self-registering minimum thermometer, placed in air at the height of 4 feet and protected from radiation, above that of a similar thermometer fully exposed to the sky, placed														Minimum reading of a thermometer at 4 feet high protected from radiation.
		On long grass exposed to ¾ths of the sky.	On long grass.	On short grass.	On garden mould.	In air one inch high.	In air three inches high.	In air six inches high.	On white raw wool.	On fine flax.	On coarse flax.	On white unwrought cotton wool.	On lead.	On blackened tin.	On white tin.	In focus of metallic parabolic reflector.	
	d h	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
1843.	May 5 21	2·4	......	0·9	1·4	2·7	2·7	......	......	......	......	......	......	......	......	2·0	50·2
	17 21	2·0	......	0·0	1·8	4·0	4·0	......	......	......	......	......	......	......	......	4·5	44·0
	18 21	1·7	......	−0·9	0·7	1·5	2·9	......	......	......	......	......	......	......	......	3·8	44·8
	30 21	3·3	......	1·9	2·2	3·0	3·1	......	......	......	......	......	......	......	......	2·8	50·3
	June 7 21	2·1	......	1·1	1·2	3·9	1·8	......	......	......	......	......	......	......	......	1·4	49·9
	12 21	...	......	......	......	0·8	......	......	......	......	......	......	......	......	......	2·2	48·8
	17 21	0·6	......	1·6	−1·9	3·0	2·1	......	......	......	......	......	......	......	......	2·0	50·6
	July 12 21	1·3	......	1·1	......	1·1	0·9	......	......	......	......	......	......	......	......	1·8	55·3
	28 21	0·2	......	0·2	......	−0·1	−0·2	......	......	......	......	......	......	......	......	1·1	53·1
	Aug. 23 21	0·7	......	1·3	......	1·4	1·2	......	......	......	......	......	......	......	......	3·9	53·9
	29 21	2·3	......	1·2	......	1·7	2·2	......	......	......	......	......	......	......	......	3·8	61·0
	Oct. 8 21	......	0·7	......	......	......	......	......	......	......	......	......	......	......	......	0·9	48·5
	10 21	......	3·3	0·9	......	0·9	......	......	......	......	......	......	......	......	......	0·7	46·9
	20 21	......	6·9	0·6	......	−0·5	......	1·7	6·3	5·8	5·9	2·8	......	......	......	3·8	38·3
	30 21	......	0·3	1·8	......	1·6	......	......	......	......	......	......	......	......	......	1·6	51·1
	31 21	......	......	0·2	......	0·0	......	......	......	......	......	......	......	......	......	1·0	42·5
	Nov. 2 21	......	−1·3	−0·3	......	0·3	......	......	0·6	0·2	−0·1	−0·1	......	......	......	−0·1	41·2
	15 21	......	1·6	0·3	......	......	......	......	3·4	−0·4	0·2	1·3	......	......	......	1·8	31·6
	17 21	......	2·5	2·7	......	......	......	......	3·5	3·7	3·1	2·5	......	......	......	2·7	37·7
	21 21	......	4·1	4·3	......	......	......	......	4·6	......	......	......	......	......	......	3·6	52·6
	22 21	......	2·4	2·1	......	......	......	......	3·5	......	......	......	......	......	......	2·8	44·6
	25 21	......	−0·3	−4·1	......	......	......	......	−0·1	−0·1	......	......	......	......	......	0·1	45·9
	30 21	......	1·6	......	......	......	......	......	......	......	......	......	......	......	......	1·2	45·7
	Dec. 3 21	......	2·3	2·3	......	......	......	......	2·1	......	2·0	......	......	......	......	3·3	45·3
	4 21	......	1·9	2·2	......	......	......	......	1·7	......	1·5	......	......	2·1	2·2	2·8	45·5
	9 21	......	2·5	−1·9	......	......	......	......	0·6	......	1·1	......	......	0·8	1·1	2·6	35·1
	11 21	......	1·1	1·4	......	......	......	......	0·3	......	2·2	......	......	0·4	0·2	2·9	30·4
	12 21	......	3·8	3·4	......	......	......	......	2·8	......	5·3	......	......	1·6	4·3	2·8	25·6
	18 21	......	2·5	3·2	......	......	......	......	2·4	......	3·1	......	......	2·5	2·7	3·7	42·5
	19 21	......	0·6	0·0	......	......	......	......	1·1	......	2·0	......	......	1·3	2·3	2·6	42·3
	21 21	......	2·9	2·7	......	......	......	......	2·2	......	2·3	......	......	1·8	2·1	3·9	41·9
	22 21	......	0·5	1·2	......	......	......	......	−0·3	......	0·9	......	......	0·3	0·9	2·3	42·5
	26 21	......	1·5	2·5	......	......	......	......	0·9	......	1·5	......	......	1·5	1·9	2·7	42·5
	28 21	......	1·8	2·0	......	......	......	......	1·9	......	1·6	......	......	1·2	1·4	2·3	42·5

Table (Continued).

The sky covered with a low cirrostratus cloud during the night previous to reading the instruments.																	
Year.	Month, day and hour of reading the instruments.	The excess of the reading of a self-registering minimum thermometer, placed in air at the height of 4 feet and protected from the effects of radiation, above that of a similar thermometer fully exposed to the sky, placed															Minimum reading of a thermometer at 4 feet high protected from radiation.
		On long grass exposed to $\frac{3}{4}$ths of the sky.	On long grass.	On short grass.	On garden mould.	In air one inch high.	In air three inches high.	In air six inches high.	On white raw wool.	On fine flax.	On coarse flax.	On white unwrought cotton wool.	On lead.	On blackened tin.	On white tin.	In focus of metallic parabolic reflector.	
	d h	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
1844.	Jan. 9 21	......	1·7	0·9	......	......	......	......	1·7	0·2	......	......	......	0·9	0·5	0·2	30·7
	12 21	......	1·8	1·7	......	......	......	......	2·4	1·2	......	......	......	2·4	1·9	−0·8	39·2
	13 21	......	1·6	0·9	......	......	......	......	1·5	0·7	......	......	......	1·7	1·2	−1·8	32·7
	Feb. 21 21	......	0·3	0·5	......	......	......	......	1·4	−0·2	......	......	0·4	......	......	−1·5	32·0
	Mar. 14 21	......	0·5	1·1	......	......	......	......	0·7	2·8	......	......	1·7	......	......	−0·2	38·1
	22 21	......	1·3	1·1	......	......	......	......	1·5	2·5	......	......	1·1	......	......	−1·2	39·8
	31 21	......	0·6	0·2	......	......	......	......	2·0	0·2	......	......	−0·2	......	......	0·7	37·3
	April 26 21	......	0·6	0·1	......	......	......	......	2·0	1·3	......	......	0·4	......	......	1·1	46·1
	May 22 21	......	0·2	0·0	......	......	......	......	......	......	......	......	......	......	......	0·4	47·4
	Sept. 9 21	......	0·8	1·1	......	......	......	......	......	0·8	......	......	......	......	......	5·8	55·8
	Oct. 15 21	......	2·0	1·7	......	......	......	......	......	2·2	......	......	......	......	......	2·9	46·7
	24 21	......	1·1	0·5	......	......	......	......	......	1·9	......	......	......	......	......	2·7	47·2
	29 21	......	3·7	2·4	......	......	......	......	......	2·7	......	......	......	......	......	7·7	42·7
	Nov. 12 21	......	0·8	0·9	......	......	......	......	......	−0·1	......	......	......	......	......	1·8	48·8
	17 21	......	0·8	0·6	......	......	......	......	......	0·6	......	......	......	......	......	2·7	48·7
	Dec. 1 21	......	1·9	0·8	......	......	......	......	......	2·1	......	......	......	......	......	3·9	34·1
	2 21	......	3·0	0·4	......	......	......	......	......	2·3	......	......	......	......	......	5·1	32·3
	3 21	......	2·2	1·1	......	......	......	......	......	1·6	......	......	......	......	......	3·6	34·1
	8 21	......	2·3	2·8	......	......	......	......	......	−0·4	......	......	......	......	......	3·9	25·1
	9 21	......	1·7	0·3	......	......	......	......	......	8·5	......	......	......	......	......	3·3	27·8
	10 21	......	1·2	−0·2	......	......	......	......	......	2·7	......	......	......	......	......	3·0	27·7
	12 21	......	0·7	−0·8	......	......	......	......	......	0·4	......	......	......	......	......	2·9	23·2
	13 21	......	0·3	−0·9	......	......	......	......	......	2·9	......	......	......	......	......	2·2	24·4
	14 21	......	2·8	1·4	......	......	......	......	......	2·3	......	......	......	......	......	2·7	28·1
	16 21	......	1·1	0·4	......	......	......	......	......	0·3	......	......	......	......	......	2·9	37·1
	17 21	......	2·0	1·1	......	......	......	......	......	1·1	......	......	......	......	......	3·1	38·1
	18 21	......	0·8	0·3	......	......	......	......	......	0·3	......	......	......	......	......	2·3	40·3
	23 21	......	2·4	0·7	......	......	......	......	......	1·5	......	......	......	......	......	4·0	29·5
	24 21	......	2·3	1·8	......	......	......	......	......	2·3	......	......	......	......	......	2·3	31·3
	25 21	......	1·8	0·8	......	......	......	......	......	1·3	......	......	......	......	......	3·4	31·1
	27 21	......	3·6	0·9	......	......	......	......	......	3·8	......	......	......	......	......	2·6	29·8
	28 21	......	1·7	1·0	......	......	......	......	......	1·3	......	......	......	......	......	3·2	40·5
	29 21	......	0·5	−1·1	......	......	......	......	......	−0·5	......	......	......	......	......	2·2	37·5

BLE CVI.—The sum of the excesses, and the mean excess of a self-registering minimum thermometer placed in Air at the height of four feet above the soil and protected from the effects of radiation, above those of similar thermometers placed on different substances, or in different situations, fully exposed to the sky.

Situation of the thermometer.	The sum of the excess of the readings of the thermometer in air above the readings of other thermometers placed as stated. General state of the sky. Cloudy.		Principally cloudy.		Half-cloudy.		Principally clear.		Cloudless.		The mean excess. Mean state of the sky.				Sum and mean excess, at times when the sky was covered with cirrostratus cloud during the night whose Height was from 3000 to 4000 feet.			Whose height was from 1000 to 2000 feet.			Difference corresponding to a difference of height of about 2000 feet.	Sky was covered with cloud all night, but not of the cirro-stratus character.	
	Number of nights.	Sum of excesses.	Number of nights.	Sum of excesses.	Number of nights.	Sum of excesses.	Number of nights.	Sum of excesses.	Number of nights.	Sum of excesses.	Whole number of nights.	Whole sum of excesses.	Mean excess.	Relative excess, that of long grass being =1000.	Number of nights.	Sum of excesses.	Mean excess.	Number of nights.	Sum of excesses.	Mean excess.		Number of nights.	Mean excess.
		°		°		°		°		°		°	°			°	°		°	°	°		°
long grass exposed to ⅔ths of the sky...	29	95·6	25	101·2	22	88·6	37	178·5	22	143·6	135	607·5	4·50	715	12	63·9	5·3	10	16·6	1·7	3·6		
long grass fully exposed..............	129	446·1	63	431·6	77	559·1	77	374·4	121	1123·2	467	2934·4	6·29	1000	30	170·8	5·7	55	92·7	1·6	4·1	4	9·7
short grass............	156	441·1	89	485·1	99	620·9	116	838·4	145	1188·2	605	3573·7	5·91	939	42	205·7	4·9	64	64·5	1·0	3·9	4	7·9
garden mould.........	29	40·2	16	59·7	10	39·7	17	55·6	13	73·8	85	269·0	3·17	500	7	25·8	3·7	6	5·4	0·9	2·8		
e inch above grass ...	34	103·0	28	146·0	34	202·5	52	368·3	38	341·2	186	1161·0	6·24	992	14	75·4	5·5	16	25·3	1·6	3·9		
ree inches above grass	31	111·5	29	149·0	28	168·9	41	295·1	29	214·0	158	938·5	5·94	944	12	70·4	5·9	10	20·7	2·1	3·8		
inches above grass...	...	......	...	......	3	12·5	6	18·7	1	5·2	10	36·4	3·64	579	1	2·3	2·3	1	1·7	1·7	0·6		
white raw wool......	65	257·1	47	355·9	42	307·3	49	422·3	73	879·7	276	2222·3	8·05	1280	15	84·8	5·7	26	51·3	2·0	3·7	3	12·2
fine flax	98	379·5	49	364·0	60	425·1	63	564·8	108	1270·2	378	3003·6	7·95	1264	23	152·9	7·1	37	59·8	1·6	5·5	4	11·8
coarse flax	22	67·6	8	45·4	4	25·5	12	100·2	12	131·7	58	370·4	6·39	1016	7	35·0	5·0	15	32·6	2·2	2·8	1	13·4
white unwrought otton wool	5	11·2	1	6·5	4	17·6	7	57·2	11	113·1	28	205·6	7·34	1167	1	4·7	4·7	4	6·5	1·6	3·1	1	12·2
lead......................	11	31·0	7	44·3	14	70·7	11	61·1	29	245·8	72	452·9	6·29	1000	4	19·9	5·0	5	3·4	0·7	4·3		
blackened tin.........	29	96·2	20	111·9	12	70·4	18	127·4	5	40·1	84	446·0	5·31	844	8	31·6	4·0	13	18·5	1·4	2·6	1	7·3
white tin	40	89·1	19	95·9	12	59·8	18	116·2	5	39·8	94	400·8	4·26	677	9	29·6	3·3	13	22·7	1·7	1·6	1	6·8
focus of reflector......	183	604·3	98	495·6	122	731·3	133	965·5	180	1538·2	716	4334·9	6·05	962	42	205·4	4·9	67	158·3	2·4	2·5	10	9·4

The numbers in the column of mean excess exhibit the mean difference between the reading of a self-registering minimum thermometer placed in air, at the height of four feet, and protected as much as possible from the effects of radiation, and the readings of similar thermometers placed as stated in the first column. The first results are those relating to grass; that deduced from grass exposed to three-fourths of the sky, is about three-fourths of that deduced from grass exposed to the whole of the sky; and that from long grass is larger than that from short grass. Of all the substances upon which experiments have been made with spirit thermometers, those on which the lowest readings have occurred were the filamentous, viz. wool and flax, and they were nearly alike.

The next class of bodies consisted of metals; of these lead exhibited the lowest readings in the mean; but this must have arisen from the circumstance of its having been generally used on the clearest nights only, during which there was found to be a very small difference between its readings and that of the thermometer in the reflector; in fact in all simultaneous observations it was found that the readings of the latter thermometer had no advantage over the readings of one placed on lead.

The thermometers which were placed within a few inches of the top of grass exhibited higher readings than those of the last class, and those which were from one to three inches from the top of grass, generally read the same as that in the reflector.

The reading which most nearly agreed with that in air was on garden mould, which was about a mean between that on long grass and that in the air; these observations were made on ground undisturbed; the readings would have been still nearer those in the air, had the ground been frequently disturbed so as to have been loose, as it was found in this state to admit the heat to pass more readily from beneath to the surface.

The numbers in the next column represent the relative radiating power of the several substances deduced by considering the mean result from long grass to be represented by 1000.

The following columns of the table represent the mean results for each substance on nights wholly cloudy, but the clouds of different heights. This result is very important, as it shows that the amount of radiation may be large on a wholly cloudy night, providing that the clouds be high. The differences between the results on a cloudy night when the clouds are high and when they are low, are very great; the numbers in the Table exhibit the mean difference between the results deduced when the clouds have been high, and when moderately low.

The last column shows that on a cloudy night, providing the kind of cloud be other than cirrostratus, the amount of radiation may nearly equal that on a cloudless night.

At a very early stage in the investigation it was found that the variation in the height of the clouds had a very considerable effect on the reading of a thermometer placed on any substance fully exposed to the sky, during those nights that the sky was wholly covered with an apparent uniform cirrostratus cloud.

At the Royal Observatory at Greenwich, the reflexion of the lights of London on the clouds is well seen; at times this appears as a narrow well-defined band of light, at the elevation of several degrees, and at other times as a broad diffused band; the lower limit of which is sometimes below the horizon. It was soon found that the difference between the reading of the thermometer in air and that on grass was greater, the greater the height of this band of light. On very many nights, and several times in the course of the same night, the height of the upper and lower edges of this band of reflected light was measured above the horizon, and from these observations that of the centre was determined; at the same times the readings of the thermometers in air and on grass were taken. By these means it was found that when the centre of the reflected light was above the horizon of Greenwich by—

4°, the excess of reading of the thermometer in air above that on grass was 1°·6
6°, the excess of reading of the thermometer in air above that on grass was 2°·5
8°, the excess of reading of the thermometer in air above that on grass was 3°·9

And whenever the centre of the band was in height less than 4°, the lower limit was generally below the horizon, and the height of the centre could not be determined; but at these times the differences between the readings of the two thermometers was seldom so much as a degree, and it was generally less, and frequently there was no difference.

The Cathedral of St. Paul is very nearly the centre of London, and it may be considered to be immediately under the centre of the reflected light; its distance from the Magnetic House is more than 25,700 feet; at this distance one degree subtends about 450 feet; the height of the ground where the observations were taken is about 100 feet higher than the ground at London.

From the above data, it appears that when the reflexion of the London lights was 4°, 6°, and 8° high, the distances of the clouds from the earth was 1900, 2800, and 3700 feet respectively.

A similar investigation was made by measuring the distance of the band from the cross on the cathedral, whose height above the ground is about 400 feet, and results were obtained differing from the preceding by about fifty feet; a much smaller number of observations, however, were used in deducing the latter results than were used in deducing the former.

As a difference of 0°·7 took place in the reading of a thermometer on grass for every variation of a degree in the height of the reflected lights, it follows that a difference of 1° in the readings of the thermometer on the grass indicated a difference of 630 feet in the height of the cloud.

TABLE CVII.—Showing the monthly mean temperature, and state of cloudiness of the nights, also the greatest differences and the lowest thermometrical readings each month.

Year.	Month.	Mean temperature of the month by a thermometer in air at 4 feet high, protected from radiation.	Mean reading of all the minimum readings, in the month, of the thermometer in air.	Excess of the mean reading of the minimum thermometer in air at 4 feet high, above that of a similar thermometer placed in the focus of a reflector.	Mean state of the sky with respect to the quantity of cloud during the night, 10 being quite cloudy, and 0 being quite clear.	Mean yearly excess of the reading of thermometer in air, above that in reflector.	The greatest excess, in each month, of the minimum reading of a self-registering thermometer, placed in air 4 feet above the ground and protected from radiation, above the minimum reading of a similar thermometer fully exposed to the sky, placed: In focus of metallic parabolic reflector.	On long grass	On short grass.	On white raw wool.	On flax.	On lead.	On tin.	The lowest reading in each month of the self-registering thermometer placed: In air at 4 feet high.	In focus of metallic parabolic reflector.	On long grass.	On short grass.	On white raw wool.	On flax.	On lead.	On tin
1841.	February	35·3°	31·6°	0·7°	9·0°	° ...	5·8°	° ...	° ...	° ...	° ...	° ...	° ...	18·8°	14·0°	°	°	°	°	°	°
	March	46·2	38·7	5·3	4·7	...	13·4	...	...	...	...	...	...	29·5	26·3						
	April	47·0	39·9	5·0	6·3	...	11·8	...	...	...	...	...	...	31·8	24·4						
	May	56·8	48·4	3·8	4·9	...	8·5	...	...	...	...	...	...	41·2	33·4						
	June	56·4	48·2	4·5	5·5	...	11·2	...	...	...	...	...	...	40·3	32·0						
	July	57·8	51·5	3·5	6·8	4·0	10·9	...	...	...	...	...	...	44·3	39·0						
	August	60·5	54·3	2·9	6·5	...	14·0	...	...	...	...	...	...	45·5	39·0						
	September	58·1	51·2	4·2	5·2	...	11·7	...	...	...	...	...	...	36·6	31·0						
	October	48·8	43·9	3·6	6·8	...	12·9	...	...	...	...	...	...	32·2	29·0						
	November	42·7	38·0	5·0	7·2	...	10·0	...	...	...	...	...	...	22·6	12·6						
	December	40·5	35·4	5·6	6·8	...	11·8	...	...	...	...	...	...	24·3	12·5						
1842.	January	32·9	29·3	5·3	7·8	...	12·0	...	...	...	...	...	...	23·2	12·5						
	February	40·8	36·0	4·9	7·5	...	9·3	...	...	...	...	...	...	26·4	21·4						
	March	44·9	39·2	4·5	6·9	...	8·2	...	...	...	...	...	...	29·9	23·2						
	April	45·2	37·3	5·1	4·6	...	9·3	...	...	...	...	...	...	28·0	20·5						
	May	53·2	45·0	5·0	5·2	...	9·5	...	...	...	...	...	...	36·4	30·5						
	June	62·9	52·2	7·1	4·6	...	16·0	...	...	...	...	...	...	44·7	35·0						
	July	60·2	52·1	5·8	6·3	5·3	10·4	...	...	...	...	...	...	45·5	37·5						
	August	65·4	56·3	5·1	6·5	...	8·3	...	...	...	...	...	...	47·5	41·7						
	September	56·4	49·8	3·3	6·0	...	8·0	...	...	...	...	...	...	41·1	36·5						
	October	45·4	39·3	6·2	5·8	...	14·2	...	...	...	...	...	...	28·3	17·2						
	November	42·8	39·0	6·8	7·4	...	11·9	...	...	...	...	...	...	31·1	20·6						
	December	45·0	40·2	4·6	7·3	...	9·4	...	...	...	...	...	...	30·8	24·7						
1843.	January	39·9	35·4	7·1	7·0	...	11·7	...	...	...	...	...	...	24·0	12·3						
	February	36·0	31·9	6·5	7·9	...	14·1	...	...	...	...	...	...	20·3	7·0						
	March	42·9	37·5	8·2	5·8	...	17·6	...	...	...	...	...	...	29·0	11·4						
	April	47·1	40·7	6·9	5·9	...	13·0	...	11·2	...	...	...	...	27·2	17·8						
	May	52·2	45·5	5·1	6·9	...	10·5	...	11·9	...	...	...	...	35·4	28·0	...	28·0				
	June	56·3	49·0	6·8	6·9	...	13·0	...	11·7	...	...	...	...	42·9	33·2	...	31·8				
	July	60·9	53·5	6·2	7·1	6·5	11·3	...	13·6	...	...	...	...	44·6	35·2	...	36·0				
	August	62·1	55·2	7·6	5·4	...	11·8	...	13·7	...	...	...	...	47·2	37·8	...	35·0				
	September	59·5	52·3	7·9	4·5	...	13·0	13·8	13·3	...	...	...	...	36·0	27·6	25·8	27·9				
	October	48·0	42·0	5·5	6·0	...	11·0	11·2	9·0	14·0	17·3	...	...	28·5	17·5	18·5	19·5	14·5	11·2		
	November	43·8	38·7	5·7	5·8	...	10·1	11·5	8·4	16·1	15·4	...	...	27·4	19·2	15·9	19·0	13·5	13·2		
	December	43·9	40·3	4·5	8·8	...	7·2	10·2	9·5	10·0	10·2	...	9·2	25·6	22·8	21·8	22·2	22·8	20·3	...	21·0
1844.	January	39·1	34·6	3·2	6·0	...	8·3	10·2	9·9	13·7	10·6	...	9·6	18·6	9·8	...	...	...	9·2		
	February	35·2	31·3	4·3	5·0	...	7·8	11·6	8·8	10·2	12·0	...	10·9	20·0	15·5	12·8	15·7	7·8	9·0	...	15·5
	March	41·5	35·7	4·2	5·5	...	12·1	11·4	8·4	18·9	18·1	8·6	...	24·1	12·0	15·2	16·2	9·2	9·6	15·6	
	April	51·7	41·8	6·8	2·3	...	12·4	14·9	13·6	20·4	19·9	16·4	...	33·4	22·7	22·1	21·6	16·5	18·0	25·5	
	May	52·9	44·4	5·9	4·4	5·4	8·4	13·2	12·2	16·8	14·5	9·3	...	33·9	27·0	26·5	27·8	20·0	22·2		
	June	58·4	50·5	6·0	3·5	...	10 0	12·2	11·6	13·6	12·9	10·0	...	44·5	35·2	32·0	32·5	...			
	July	61·2	54·3	6·2	4·5	...	12·9	17·3	12·8	14·2	16·8	...	...	47·1	39·3	35·5	37·0	33·8	31·2		
	August	57·7	50·3	6·4	3·7	...	9·5	11·6	10·1	12·6	13·9	...	...	42·8	34·8	31·8	32·8	...	30·8		
	September	57·9	50·2	8·0	4·6	...	13·7	13·1	12·1	...	14·6	...	...	34·8	27·2	25·0	27·5	...	22·6		
	October	49·2	43·5	5·8	4·9	...	13·7	18·8	17·8	...	21·8	...	...	30·8	25·5	24·2	26·2	...	23·5		
	November	44·0	39·6	5·2	6·5	...	9·6	11·0	8·9	...	13·4	...	...	27·4	20·2	19·3	21·8	...	18·0		
	December	33·0	30·4	2·7	7·7	...	13·7	13·5	8·7	...	17·4	...	...	21·1	7·7	7·9	12·4	...	4·0		

The power of radiation, as exhibited in this Table, has evidently no tendency to increase with the heat, and in this respect does not serve even as an approximation to the law of radiation established by the experiments of MM. Dulong and Petit, viz. that the velocity of cooling *in vacuo* (or force of radiation) increases in the terms of a geometrical progression for excess of temperature in arithmetical progression; it is most probable that the effect is masked by too many disturbing causes to enable us from mere inspection to discover the law of its progression.

Table CVIII.—Monthly mean of the minimum temperature of the Air.

Year.	Month.	The monthly mean reading of the minimum self-registering thermometer placed in air at the height of 4 feet, according to the state of the sky. State of the sky.									
		Cloudy.		Broken clouds.		Half-cloudy.		Principally clear.		Cloudless.	
		Number of nights.	Mean of the minimum temperatures.	Number of nights.	Mean of the minimum temperatures.	Number of nights.	Mean of the minimum temperatures.	Number of nights.	Mean of the minimum temperatures.	Number of nights.	Mean of the minimum temperatures.
1843.	April.	2	43°·9	1	37°·4	3	42°·8	2	38°·3	3	40°·5
	May.	12	46·9	6	46·0	4	47·0	7	43·0	2	43·1
	June.	6	51·4	9	49·8	1	47·0	7	48·0	7	47·2
	July.	5	53·3	6	55·2	7	54·1	10	49·7	2	54·2
	August.	2	57·5	2	59·4	7	59·2	12	53·5	6	52·5
	September.	3	58·2	3	54·3	7	55·2	6	48·1	11	50·8
	October.	6	44·9	3	44·4	6	50·0	8	40·4	8	34·6
	November.	9	42·5	2	49·2	7	41·6	7	39·8	6	31·0
	December.	17	40·5	7	42·8	1	40·1	5	37·6	...	......
1844.	January.	8	33·0	7	37·6	5	34·1	7	34·1	2	22·0
	February.	7	32·3	6	27·9	6	35·3	6	29·4	4	29·4
	March.	8	37·8	5	38·9	9	33·5	2	41·9	7	30·5
	April.	3	47·1	2	46·6	3	45·4	4	43·0	18	39·6
	May.	9	47·1	1	46·2	6	42·4	5	42·5	10	44·1
	June.	2	50·3	10	51·4	7	52·5	5	50·4	6	53·0
	July.	4	56·2	9	55·4	5	54·6	1	53·8	12	52·8
	August.	7	54·8	1	51·6	5	51·1	6	49·3	12	47·7
	September.	6	55·3	5	54·1	2	59·7	5	50·8	12	44·3
	October.	10	47·0	1	45·3	6	45·3	6	46·0	8	40·1
	November.	12	42·6	3	42·0	6	38·9	7	33·5	2	41·6
	December.	23	31·9	...	......	...	......	1	30·1	6	24·6

By taking the mean of these numbers according to the number of nights from which each result is deduced, we find that the mean of the lowest readings of the temperature of the air during

161 cloudy nights was .... 46°·2
89 broken cloudy nights was .... 46·9
103 half-cloudy nights was .... 46·0
119 principally clear nights was .... 43·9
144 cloudless nights was .... 42·4

INDEX.

XII. *On the Cause of the Discrepancies observed by* Mr. Baily *with the Cavendish Apparatus for determining the mean density of the Earth.*

By George Whitehead Hearn, *Esq., of the Royal Military College, Sandhurst. Communicated by Sir* J. F. W. Herschel, *Bart., F.R.S.*

Received February 11,—Read March 11, 1847.

In the Fourteenth Volume of the Transactions of the Royal Astronomical Society will be found a full account of the Cavendish apparatus, and of the mode of experimenting followed by Mr. Baily. It will therefore not be necessary for me, in this place, to enter into any detail as to the different parts of the instrument, and the various precautions adopted in order to avoid that singular source of error 'currents of air in the torsion box arising from unequal temperature,' which had been discovered by Cavendish. It will be sufficient for me to state that all the arrangements are of a highly satisfactory kind, and that I am of opinion that no aerial currents could have existed in the torsion box.

The deduction of the mean density of the earth from the observed vibrations of the balls influenced by the torsion force and the attraction of the masses, is founded on a mathematical theory of the motion of the balls given by the Astronomer Royal, Mr. Airy; and as this theory is certainly insufficient to account for the discrepancies, it will here be necessary to give a brief sketch of it.

The momentum of attraction of the masses and planks on the torsion rod and balls supposed to be in the zero position, is calculated. The weights or masses are all represented in grains, and one inch is taken as the unit of length. The momentum is called E. In deducing it, the portion depending on the attraction of the planks is obtained by supposing the masses of such planks to be collected in their axes. The moment of inertia is then found and called F. A modulus of attraction k is then assumed, so that $\frac{k\times \mathrm{E}}{\mathrm{F}}$ is the impressed angular accelerating force on the rod and balls in the zero position.

The distance from the centre of motion to the centre of the balls is denoted by c, so that, if θ be the very small arc described, $c\theta$ is the space described in inches.

Let $m^2\theta$ denote the force of torsion which is known to be proportional to the angle described, supposing the apparatus in a normal state; or if we suppose it slightly out of order so as to rest at an angle α from the zero position when the masses are removed, $m^2(\theta-\alpha)$ will denote the force of torsion; and it may also be conceived that the small force of attraction of the torsion box, &c. is included in this.

The equation for angular motion is then

$$\frac{d^2\theta}{dt^2}+m^2(\theta-\alpha)=\frac{k\times \mathrm{E}}{\mathrm{F}}(1+n\theta),$$

neglecting θ^2, &c., where n is a constant depending on the distance of the masses from the balls.

Making $m^2-\frac{k\times \mathrm{E}}{\mathrm{F}}n=\mu^2$, this equation may be written

$$\frac{d^2\theta}{dt^2}+\mu^2\theta=m^2\alpha+\frac{k\times \mathrm{E}}{\mathrm{F}}.$$

Multiply by i the distance from the centre of motion to the scale $=108$ inches, and put $i\theta=x$, $i\alpha=b$, then

$$\frac{d^2x}{dt^2}+\mu^2x=m^2b+\frac{k\times \mathrm{E}\times i}{\mathrm{F}},$$

in which the masses are in the positive position.

The integral of the equation is

$$x=\frac{m^2b}{\mu^2}+\frac{k\times \mathrm{E}\times i}{\mathrm{F}\mu^2}+\mathrm{A}\cos(\mu t+\mathrm{B}),$$

in which A and B are arbitrary constants depending on initial circumstances.

Let e be the value of x for the 'resting point,' or mean value of the above,

$$e=\frac{m^2b}{\mu^2}+\frac{k\times \mathrm{E}\times i}{\mathrm{F}\mu^2}.$$

When the masses are in the negative position, the only alteration in the equation (still measuring x the same way) is in the sign of k; we have, therefore, for the negative resting point,

$$e'=\frac{m^2b}{\mu^2}-\frac{k\times \mathrm{E}\times i}{\mathrm{F}\mu^2},$$

$$\therefore \qquad \frac{e-e'}{2}=\frac{k\times \mathrm{E}\times i}{\mathrm{F}\mu^2}.$$

Also T being the time of vibration $\mathrm{T}=\frac{\pi}{\mu}$.

Hence $$\frac{1}{k}=\frac{2\mathrm{E}i}{\mathrm{F}\pi^2}\cdot\frac{\mathrm{T}^2}{e-e'}.$$

It is then shown, on the hypothesis that k is the same for all substances, and on the law of universal gravitation, that $k\Delta=\mathrm{H}$, a constant depending on known quantities, so that we have

$$\Delta=\mathrm{H}.\frac{2\mathrm{E}i}{\mathrm{F}\pi^2}\cdot\frac{\mathrm{T}^2}{e-e'}.$$

Every division of the scale is $\frac{1}{26}$th of an inch, so that if s and s' be the scale read-

ings for the resting points

$$s=26e \quad \text{and} \quad s'=26e',$$

$$\therefore \qquad \Delta=\frac{T^2}{D}\times C,$$

where

$$D=\frac{s-s'}{2} \text{ and } C=\frac{26HEi}{F\pi^2}.$$

$\frac{26Hi}{\pi^2}$ is called the general constant, which, combined with $\frac{E}{F}$, gives C the *special* constant.

The resting points s and s' are not directly observed, and in strict accordance with the preceding theory, ought to be found thus.

Let σ and σ' be the scale readings at the extremities of the arc of vibration, or when $\mu t+B=0$ and π respectively,

$$\sigma=s+26A\cos 0=s+26A,$$
$$\sigma'=s+26A\cos\pi=s-26A;$$
$$\therefore \qquad s=\frac{1}{2}(\sigma+\sigma').$$

We will now deduce a few values of Δ in strict accordance with the theory from the numerical values given by Mr. Baily, extracting the data furnished by observation from his tables.

1st Series.—2-inch lead balls with *bifilar silk* lines; distance ·177 inch.

1841.	No.	Position of masses.	Extreme divisions observed.	Time of vibration.	Mean of times.
				sec.	sec.
	13	Positive	{50·00, 177·50}	501·782	501·434
	14	Negative......	{170·00, 27·50}	501·087	

Here $s=113·9$, $s'=98·75$; and $\therefore$ $D=\frac{s-s'}{2}=7·575$.

log C=6·360356; and $\therefore$ $\Delta=7·6103$.

Again,

22nd Series.—$2\frac{1}{2}$-inch hollow brass balls with *single copper* wire, diameter ·0219 in.

1841.	No.	Position of masses.	Extreme divisions observed.	Time of vibration.	Mean of times.
				sec.	sec.
	1084	Positive	{86·20, 118·74}	216·111	216·1105
	1085	Negative......	{117·70, 81·62}	216·110	

Here $s=102·47$, $s'=99·66$; and $\therefore$ $D=\frac{s-s'}{2}=1·405$.

log C=6·354454; and $\therefore$ $\Delta=7·5184$.

It is to be observed that Mr. Airy's theory does not contemplate a difference in the *time* of vibration in different experiments with the same balls suspended in the same manner, and moreover the effect of resistance of the air is entirely omitted.

I have therefore selected the preceding consecutive positive and negative experiments which have the times very nearly alike.

The consideration of the resistance of the air might, however, at first sight, seem very necessary. Since the velocity is very small, we may, according to the conclusions of experimentalists, assume that the resistance varies nearly as the velocity; we ought therefore to introduce the term $\varrho\frac{dx}{dt}$ into the differential equation. The result shows that the time of vibration is very little affected by the resistance, but the resting point very considerably. Mr. BAILY observes four extreme divisions, that is, he watches the motion for three successive vibrations, and then takes the means of successive pairs of extreme divisions. In this way he has three first means. He then takes the means of successive pairs of those first means, and thus obtains two second means; and lastly, he takes the mean of the second means, and this third mean is considered the resting point or place where the torsion rod would be in equilibrium. This proceeding, as I afterwards shall prove, does in general pretty accurately correct for the resistance of the air, and therefore we may consider BAILY's 'resting points' as those which would arise from Mr. AIRY's theory, supposing the term $\varrho\frac{dx}{dt}$ for resistance had been introduced into his investigation. After this Mr. BAILY combines his experiments in a very remarkable manner. Three successive experiments are combined in order to produce what he terms a single result; and in combining them the means of the positive and negative resting points are taken, and *means* also of the *times* of vibration; for such times it is found are never all exactly alike, and sometimes differ considerably. In order that the reader may at once see the method pursued, I have made the following extract from the tables:—

3rd Series.—2-inch lead balls with *bifilar silk* lines; distance $=\cdot177$ inch; upper distance ·158 inch; lower dist. ·197 inch.

1841.	No.	Position of the masses.	Extreme divisions observed.	1st Mean.	2nd Mean.	3rd Mean or resting point.	Observed times.		2 N.		N for mean resting point.
							h m s	h m s	m s	m s	s
Feb. 26. T = 44·500 B = 29·880	113	Negative.	126·90 54·80 119·00 56·90	90·850 86·900 87·950	88·875 87·425	88·150	At div. 85 10 13 42·5 21 24 30 11	At div. 90 10 13 20 21 48 29 45	By div. 85 16 28·5 8 14·25	By div. 90 16 25 8 12·50	493·148
	114	Positive.	56·90 155·40 63·80 152·00	106·150 109·600 107·900	107·875 108·750	108·312	At div. 105 10 38 35 47 24 55 26·5	At div. 110 10 38 51·5 47 6 55 45	By div. 105 16 51·5 8 25·75	By div. 110 16 53·5 8 26·75	506·412
	115	Negative.	152·00 30·60 146·80 36·00	91·300 88·700 91·400	90·000 90·050	90·025	At div. 85 11 4 33 12 24 21 20	At div. 90 11 4 19·5 12 38 21 5·5	By div. 85 16 47 8 23·5	By div. 90 16 46 8 23	502·998

3rd Series.—2-inch lead balls with *bifilar silk* lines; distance $=\cdot177$ inch.

1841.	No.	Pos.	Observed			Deduced.			Results.		
			Time of vibration.	Resting point.	Distance.	Time = N.	Deviation = D.	Distance = δ.	Single.	Daily.	
Feb. 26.	113 114 115	− + −	493·148 506·412 502·998	88·150 108·312 90·025	10·9936 11·0008	502·242	9·612	10·9972	6·0199	5·6946	log. C = 6·360356.

It is true that results tolerably accordant on the whole are deduced in this manner, but there is no explanation why they should be so combined. It may be regarded as a mode of combination of the experimental data so arranged as to allow a medium result to emerge, in spite of the immense discrepancies which he could not but have perceived would have appeared had results been deduced from every pair of successive experiments. I must not here be understood as insinuating any kind of deception on the part of this great experimental philosopher; on the contrary, he is candid in the extreme. He frequently refers to the "variation in the time of vibration and the perturbation of the resting points" as things for which he cannot account, and sometimes says, "there must be some disturbing force." Also "the force of torsion must be subject to variation." With respect to the hypothesis of a change in the torsion force, though I might grant its possibility when a single wire is employed for the suspending line, I cannot grant it when bifilar silk lines are used. In this case the force of torsion is easily calculated, and there is no reason why it should at all change. But when such lines, ·177 inch apart, are used, the greatest anomalies occur; and I am persuaded that if the distance were still further diminished, and the torsion force thereby rendered weaker, the anomalies would be still further increased.

In order to obtain a theory which would account for the anomalies I tried many plans. One was the integration of the equation of motion (including resistance) to terms of the second order, but the corrections thence arising were far too minute to afford any explanation. Besides the above reason against the alteration of the torsion force, another occurred to me, which was this,—were a change in the torsion force the sole cause of the perturbations, the time and resting point ought always to change *simultaneously*, but this is observed not to be the case. Examples in abundance will present themselves to any one consulting the tables, in which the time changes, and the resting point remains nearly unaltered, and *vice versâ*. Heat also was out of the question, in consequence of the extreme precautions used to prevent the intrusion of this sort of disturbance.

At length I determined to try the effect of a supposed *magnetic* state of the masses and balls, and, as will be seen in the sequel, the hypothesis succeeded beyond my most sanguine expectations. It is strongly suspected that all bodies are more or less susceptible of the magnetic state, and I think it very probable that what is called the coercive power of a substance, or that power which it possesses of retaining its magnetic state, after the magnetizing power has been withdrawn, for a longer or shorter period, may not only differ for different substances, which we know it does, but for different intensities of magnetization. Thus, if magnetism be induced by a powerful magnet in a mass of soft iron or lead, the magnetic state will, to all appearance, subside when the magnet is withdrawn, and that very rapidly, perhaps instantaneously. Now I contend that such may not be the case if the magnetization were very small; it may require, even in lead, some time to elapse before the very feeble magnetic state wholly subsides.

We are sure that some degree of magnetism must be induced in the masses and balls by terrestrial magnetic influence, and then the magnetic systems may be capable of sensibly influencing each other. Now when balls are magnetized, their action is the same as if there were a small magnet concentric with each ball, and therefore the mutual action of balls on each other is reduced to that of small magnets, the distance of whose centres is great compared with their lengths. In the case of the earth being one of the magnetic bodies, the distance of the centres may be well considered infinite in comparison with the lengths of the supposed magnets, and therefore magnetic action of the earth upon any body at its surface will not tend to produce any motion of translation, or to increase the *weight* of such body. But when two balls at a moderate distance from each other are magnetized, the force producing motion of translation does not vanish, except for certain relative positions of the magnetic axes, and is sometimes positive and sometimes negative; that is, sometimes attractive and sometimes repulsive. Imagine then that the masses have remained in the positive position for some hours (*e. g.* during the night); the mutual influence of the masses and balls combined with terrestrial influence must have induced a certain very minutely magnetic state in those bodies, and the resulting attraction of the masses and balls is mixed up with the attraction of their mutual gravitation. Now reverse the masses by turning the plank round the centre of motion through nearly 180°, the action arising from magnetism, if we suppose the masses capable of preserving their minutely magnetic state, will now probably be one of repulsion, though perhaps not precisely equal in amount to the magnetic attraction in the positive position. This repulsion will also be mixed up with the action of mutual gravitation.

Now, when we reflect that the attraction arising from gravitation between a mass and one of the balls is exceedingly minute (about the tenth of a grain), it is clear that an almost inconceivably feeble magnetic state may be the cause of great perturbations.

I now proceed to apply mathematics to these views. The term depending on the action of gravitation being $\frac{k\mathrm{E}}{\mathrm{F}}\, i.\varphi x$, let that depending on magnetic action be $\frac{k\mathrm{M}}{\mathrm{F}}\, i.\psi x$, then the differential equation of motion will be

$$\frac{d^2x}{dt^2}+\varrho\frac{dx}{dt}+m^2(x-b)=\frac{k\mathrm{E}i}{\mathrm{F}}.\varphi x+\frac{k\mathrm{M}i}{\mathrm{F}}.\psi x;$$

and if we suppose φx and ψx expanded to the first power of x, we may write

$$\varphi x=1+ax,\ \psi x=1+cx;$$

and making

$$m^2-\frac{k\mathrm{E}ia}{\mathrm{F}}-\frac{k\mathrm{M}ic}{\mathrm{F}}=\mu^2,$$

we have

$$\frac{d^2x}{dt^2}+\varrho\frac{dx}{dt}+\mu^2x=m^2b+\frac{ki(\mathrm{E}+\mathrm{M})}{\mathrm{F}}. \qquad (1.)$$

Let P be the value of x for the resting point or position of equilibrium, then

$$\mu^2\mathrm{P}=m^2b+\frac{ki(\mathrm{E}+\mathrm{M})}{\mathrm{F}}, \qquad (2.)$$

and the equation of motion may be written

$$\frac{d^2x}{dt^2}+\varrho\frac{dx}{dt}+\mu^2(x-\mathrm{P})=0.$$

The integral of this equation is

$$x=\mathrm{P}+\varepsilon e^{-\frac{\varrho t}{2}}\left\{\cos yt+\frac{\varrho}{2y}\sin yt\right\}, \quad \ldots \ldots \ldots \ldots \quad (3.)$$

where $y^2=\mu^2-\frac{1}{4}\varrho^2$,

and $$\frac{dx}{dt}=-\frac{\mu^2\varepsilon}{ye^{\frac{\varrho t}{2}}}\sin yt \quad \ldots \ldots \ldots \ldots \ldots \quad (4.)$$

Hence the time of vibration $=\frac{\pi}{y}$.

To determine the coefficients from observation. Let A, B, C be the extreme divisions observed, and let $e^{-\frac{\varrho\pi}{2y}}=u$. Then

$$\mathrm{A}=\mathrm{P}+\varepsilon,$$
$$\mathrm{B}=\mathrm{P}-\varepsilon u,$$
$$\mathrm{C}=\mathrm{P}+\varepsilon u^2,$$
$$\therefore \quad \mathrm{P}=\frac{\mathrm{AC}-\mathrm{B}^2}{\mathrm{A}+\mathrm{C}-2\mathrm{B}}, \quad u=\frac{\mathrm{B}-\mathrm{C}}{\mathrm{B}-\mathrm{A}}.$$

Now in all cases it is observed that B−C and B−A do not greatly differ, so that $u=1-v$, where v is a small quantity,

or $$e^{\frac{\varrho \mathrm{T}}{2}}=1+v, \quad \therefore \varrho=\frac{2}{\mathrm{T}}\log_e(1+v)=\frac{2v}{\mathrm{T}} \text{ nearly},$$

$$\therefore \quad \mu^2=y^2+\frac{1}{4}\varrho^2=\frac{\pi^2}{\mathrm{T}^2}+\frac{v^2}{\mathrm{T}^2}=\frac{\pi^2}{\mathrm{T}^2}\left(1+\frac{v^2}{\pi^2}\right).$$

In this the masses are supposed in the positive position: when they are in the negative position, if we suppose x measured from the zero point in the contrary direction, and that M becomes M′, and c becomes c', &c., we have

$$\mu'^2\mathrm{P}'=-m^2b+\frac{ki(\mathrm{E}+\mathrm{M}')}{\mathrm{F}}, \quad \ldots \ldots \ldots \ldots \quad (5.)$$

where $$\mu'^2=\frac{\pi^2}{\mathrm{T}'^2}\left(1+\frac{v'^2}{\pi^2}\right).$$

In our ignorance as to the mutual positions of the magnetic axes we do not know the precise relation of M′ to M, but as a probable and at the same time simple approximation, suppose $\mathrm{M}'=-\mathrm{M}$.

Then by adding (2.) and (5.) we have

$$\frac{2ki\mathrm{E}}{\mathrm{F}}=\mu^2\mathrm{P}+\mu'^2\mathrm{P}'.$$

Now $k\Delta=\mathrm{K}$, a known constant,

$$\therefore \quad \frac{2\mathrm{K}i\mathrm{E}}{\Delta\mathrm{F}}=\frac{\pi^2\mathrm{P}}{\mathrm{T}^2}\left(1+\frac{v^2}{\pi^2}\right)+\frac{\pi^2\mathrm{P}'}{\mathrm{T}'^2}\left(1+\frac{v'^2}{\pi^2}\right).$$

The corrections $\frac{v^2}{\pi^2}$ and $\frac{v'^2}{\pi^2}$ are found to be insignificant, and therefore

$$\frac{2}{\Delta}=\frac{P}{T^2}C^{-1}+\frac{P'}{T'^2}C^{-1},$$

where $C=\frac{KiE}{F\pi^2}=$ BAILY's special constant.

Hence if $\frac{1}{\delta}=\frac{P}{T^2C}$ or $\delta=\frac{T^2}{P}C,$

and $\frac{1}{\delta'}=\frac{P'}{T'^2C}$ or $\delta'=\frac{T'^2}{P}C,$

we have $\frac{2}{\Delta}=\frac{1}{\delta}+\frac{1}{\delta'},$

$$\therefore \quad \Delta=\frac{1}{\frac{1}{2}\left(\frac{1}{\delta}+\frac{1}{\delta'}\right)}.$$

I will now apply this method to a few examples, but instead of deducing $\frac{1}{\delta}$ and $\frac{1}{\delta'}$ from two consecutive positive and negative experiments, I will usually employ their mean values deduced from all the experiments made during a day.

Also, since $A-C$ is usually very small compared with $A+C-2B$, we have $AC=\frac{1}{4}(A+C)^2$ nearly, and therefore

$$P=\frac{AC-B^2}{A+C-2B}=\frac{1}{4}\cdot\frac{(A+C)^2-4B^2}{(A+C)-2B}=\frac{1}{4}\left\{A+C+2B\right\}=\frac{1}{2}\left\{\frac{1}{2}(A+B)+\frac{1}{2}(C+B)\right\},$$

which is BAILY's method, and therefore I shall take BAILY's mean resting point as sufficiently near the truth.

2-inch lead balls with *bifilar silk* lines ·177 inch distant, February 26, 1841. Experiment 113, negative

$$\begin{aligned} \log\ C&=6\cdot360356 \\ \log\ T'^2&=5\cdot385954 \\ \text{ar. co. } \log\ P'&=8\cdot926281 \\ \hline &\ \ \cdot672591 \\ \hline \log\frac{1}{\delta'}&=9\cdot327408 \end{aligned} \qquad \begin{aligned} P'&=\ 11\cdot85 \\ T'&=493\cdot148 \\ \\ \\ \\ \therefore\ \frac{1}{\delta'}&=\cdot21747. \end{aligned}$$

Experiment 114, positive

$$\begin{aligned} \log\ C&=6\cdot360356 \\ \log\ T^2&=5\cdot409010 \\ \text{ar. co. } \log\ P&=9\cdot080294 \\ \hline &\ \ \cdot849660 \\ \hline \log\frac{1}{\delta}&=9\cdot150339 \end{aligned} \qquad \begin{aligned} P&=\ \ 8\cdot312 \\ T&=506\cdot412 \\ \\ \\ \\ \therefore\ \frac{1}{\delta}&=\cdot14136 \end{aligned}$$

$$\therefore\ \frac{1}{2}\left\{\frac{1}{\delta}+\frac{1}{\delta'}\right\}=\cdot17941=\frac{1}{\Delta}, \qquad \therefore\ \Delta=5\cdot5738.$$

29th Series.—2-inch zinc balls with *bifilar silk* lines ·367 inch distant, December 24th, 1841. By pursuing the same mode of calculation, log C=6·358224, I find as follows:—

Pos. Expts.	$\frac{1}{\delta}$.	Neg. Expts.	$\frac{1}{\delta'}$.
1357	·68293	1358	— ·31984
1359	·58530	1360	— ·18163
1361	·55658	1362	— ·25055
1363	·65864	1364	— ·34102
	4)2·48345		4)—1·09304
	·62086		— ·27326
	—·27326		
	2)·34760		

$$\cdot 17380=\frac{1}{\Delta}, \qquad \therefore \quad \Delta=5\cdot 7537.$$

On referring to the investigation, it will be seen that neglecting the small quantity m^2b*, $\frac{1}{\delta}-\frac{1}{\delta'}$, by equations (2.) and (5.) is a measure of the magnetic action during two consecutive positive and negative experiments. Accordingly we have the following table:—

Experiments.	Quantities proportional to magnetic action.
1357, 1358	1·00277
1358, 1359	·90514
1359, 1360	·76693
1360, 1361	·73821
1361, 1362	·80713
1362, 1363	·90919
1363, 1364	·99966

The apparatus is here assumed to have been in a normal state, or very nearly so.

52nd Series.—2½-inch lead balls with *single copper wire*, dist. ·0178 inch, March 20, 1842.

Pos. Expts.	$\frac{1}{\delta}$.	Neg. Expts.	$\frac{1}{\delta'}$.
1893	·27914	1894	·09847
1895	·24245	1896	·12632
	2)·52159		2)·22479
	·26079		·11239
	·11239		
	2)·37318		

$$\cdot 18659=\frac{1}{\Delta}, \qquad \therefore \quad \Delta=5\cdot 3593.$$

* m^2b must be very insignificant; for no experimenter would suffer his apparatus to be so much out of order as to render it very sensible.

Experiments.	Quantities proportional to magnetic action.
1893	
	·18067
1894	
	·14398
1895	
	·11613
1896	

Supposing the apparatus in the normal state.

I think it is to be regretted that in Mr. Baily's experiments he should have so seldom mentioned in what position the masses had been left during the night; this last example is one of the cases in which he distinctly mentions that the masses had remained in the positive position during the night, and the first observation next morning was taken from the *spontaneous* motion of the rod. Accordingly we find the magnetic action positive, and diminishing subsequently, as we would have been led to expect from the circumstances.

The following is an instance in which the magnetic action is negative, and I have placed in juxtaposition the gravitating energy.

Dec. 8, 1841. Expts.	Magnetic action. $\frac{1}{\delta}-\frac{1}{\delta'}$.	Gravitating action. $\frac{1}{\delta}+\frac{1}{\delta'}$.
1229		
	−·59781	·36883
1230		
	−·61569	·35095
1231		
	−·63164	·36690
1232		
	−·63573	·36281
1233		
	−·64200	·36908
1234		
	−·65325	·35783
1235		
	−·66877	·37335
1236		
	−·69288	·34924
1237		
	−·71039	·36675
1238		
	−·72582	·35132
1239		
	−·74456	·37006
1240		
	−·76577	·34885
1241		
	−·79827	·38135
1242		
	Mean	·37090
		$·18545=\frac{1}{\Delta}$.

Hence $\Delta=5·3923$.

Again, from the same series of experiments:—

Experiments.	Magnetic action. $\frac{1}{\delta}-\frac{1}{\delta'}$.	Gravitating action. $\frac{1}{\delta}+\frac{1}{\delta'}$.
1243		
	2·70630	·38762
1244		
	2·70244	·38376
1245		
	2·70090	·38530
1246		
	2·69498	·37938
1247		
	2·71824	·35612
1248		
	2·72141	·35929
1249		
	2·71262	·36808

The two last examples are taken from the 25th series, 2-inch lead balls with single copper wire, diam. ·0219 inch; and what is very remarkable is, that the time of vibration remains throughout nearly the same, 253 seconds. And since

$$\frac{\pi^2}{T^2}=m^2-\frac{k\mathrm{E}ia}{\mathrm{F}}-\frac{k\mathrm{M}ic}{\mathrm{F}},$$

this circumstance shows two things, first that the term $\frac{k\mathrm{M}ic}{\mathrm{F}}$ must be exceedingly small, and therefore c very small; and also that m^2, and therefore the force of torsion is not sensibly changed.

I have therefore, I conceive, satisfactorily shown that the masses and balls do exert influences on each other independent of the action of gravitation, and that such influences are of a very fluctuating nature, and the action arising from them is either positive or negative; and changes as to sign when the masses are turned round a vertical axis through 180°, or thereabouts. Moreover, that such action may either fall short of that arising from gravitation, or exceed it many times.

It is inconceivable that this disturbing force can arise from anything but magnetic influence, and in this we must remember there are three distinct modifying causes at work,—first, terrestrial influence; second, mutual influence of masses and balls; and third, the alternate motion of the masses changing from one position to the other. I am of opinion that ordinary magnetic influence is inadequate to the explanation of the motions, or rather of the disturbing force demonstrated to have an existence, and that, as all the substances used are such as are classed by Dr. Faraday amongst the *diamagnetic*, that new magnetic condition discovered by this illustrious experimentalist is also greatly concerned as a cause. It will probably be found that both species of magnetism combine in producing such very extraordinary results. The circumstance of the numbers proportional to the gravitating influence not exactly agreeing is easily explained. The simple condition $\mathrm{M}+\mathrm{M}'=0$, which we have assumed, is of course not accurate, and the wonder is that it answers so well.

But we now come to the question how future experiments with the torsion balance are to be conducted so as to arrive at a satisfactory conclusion as to the mean density Δ. It has occurred to me that, instead of using diamagnetic substances, we should have hard iron balls possessing the ordinary magnetic state in sufficient intensity to render their magnetic effect sensible, so that we may with precision ascertain the magnetic axes of each iron mass and ball. Suppose the balls placed on the rod so that their magnetic axes shall be in the direction of the rod and therefore horizontal. Let the rod be suspended in the magnetic meridian, and let the masses be placed with their magnetic axes vertical, and centres in the same horizontal plane with those of the balls. The contiguous masses and balls would exert no magnetic force on each other perpendicular to the length of the rod, and the resolvent of magnetic force of a further mass on a nearer ball, or a further ball on a nearer mass per-

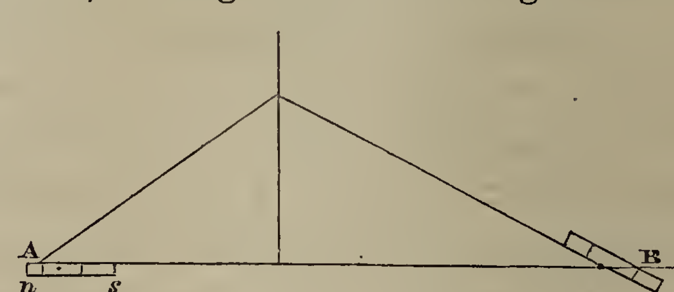
pendicular to the rod, would be exceeding small, and might possibly by some contrivance be altogether counteracted. Under such an arrangement the only effective force would be that of gravitation. I would also do away with the planks, support the masses on square blocks of wood, and transport them from the positive and negative positions by means of wooden tram-roads parallel to the torsion box, so that all motion round an axis might be avoided.

It has also struck me that by suspending the rod by means of a single silk thread having little or no power of torsion, and by having very delicate hydrometers at each end of the torsion rod, the stems of which are attached to very fine hairs or silken threads passing over small fixed pulleys in the horizontal plane of the torsion rod, the horizontal portions of the hairs or threads being affixed to the ends of the torsion rod, the experiments might be rendered purely statical. Supposing the hydrometers just floating in their position of equilibrium when the rod is in its zero position, and those hydrometers on contrary sides of the torsion rod opposed to the masses, it is clear that when the rod moves towards the masses, and raises the hydrometers above their position of equilibrium, that the tensions of the threads would increase, and vary as the angle through which the torsion rod has moved. These forces of tension would ultimately be in equilibrium with the gravitating action, and by observing this position of equilibrium the force of tension and therefore that of gravitation would become known. There may be practical objections to this arrangement, but I am of opinion that by using proper substances for the stems, &c. of the hydrometers, and proper care in the manufacture of the small pulleys, they might be overcome.

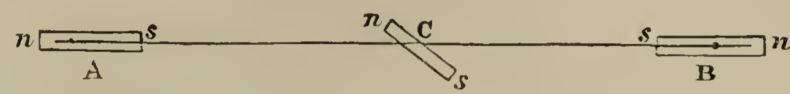
After my views were fully matured I had some correspondence with Sir JOHN HERSCHEL, to whom I in part detailed them, and with the kindness and urbanity which so eminently distinguish him, he undertook to lay my communication before the Royal Society: in one of his letters he thus expresses himself.

"Very many years ago, immediately after the publication of a joint paper by Mr. BABBAGE and myself on the magnetic action of revolving copper discs, &c. on magnets, a course of experiments suggested itself to me which, for want of a proper *locale* where I could establish an apparatus of considerable dimension and great delicacy out of the way of currents of air, I did not execute, a thing I now much regret.

"The plan of these experiments was to attach to the two arms of a long torsion balance two magnets, a stronger and a somewhat weaker; thus A being the weaker and B the stronger, A horizontal and B so inclined as *precisely* to counteract and destroy the directive power of A (by forming 'neutral couples'), the line A B being in the magnetic meridian, which would always be practicable as the superior power of B



might be weakened in any arbitrary degree by inclining it, or other modes of making the combination astatic, as for example by placing A and B (supposed equal or very nearly so) horizontal, and adding a small adjustable correcting magnet at C, &c. &c. &c.



"The whole being then inclosed in a case, it was proposed to bring near to both A and B (and on opposite sides) masses of various metals and other substances not usually considered as magnetic, with a view to increase by leverage and so place in evidence any very minute magnetic forces which might reside in the substances used.

"I think that such a course of experiments would not now be without its interest, and that the magnetic and diamagnetic powers of Faraday would be exhibited in such a course perhaps under new and remarkable light."

I have now only further to add, that I think it would very much conduce to our knowledge of the subject to make a new series of experiments with the Cavendish apparatus as used by Mr. Baily, but in this manner:—Carefully stating how the masses of balls were disposed previously to the commencement of the experiments, and continuing those experiments without interruption day and night by several observers relieving each other through three or four successive days and nights, with a view to ascertaining the periodicity, &c. of the magnetic or diamagnetic forces.

Of course when real magnets are attached to the rod, as I have advised when the object of investigation is the earth's mean density, it would be necessary to render the torsion rod astatic by Sir John Herschel's ingenious device, which, as he states, was contrived before he had had any knowledge of M. Nobili's astatic combination of needles.

XIII. *Electro-Physiological Researches.—Fifth Series.* Part I. *Upon Induced Contractions. By Signor* Carlo Matteucci, *Professor in the University of Pisa, &c. &c. Communicated by* Michael Faraday, *Esq., &c. &c.*

Received May 20,—Read June 10, 1847.

In my third memoir upon Induced Contractions, published in the Philosophical Transactions for 1845, at p. 303, after having discussed at length the various hypotheses which appear to offer an explanation of this phenomenon, I was led to conclude that it was due to nervous influence acting through the muscle during contraction; that, in a word, it was to be referred to a kind of nervous induction. In effect, I detailed a number of experiments in that memoir, which prove that there is never any manifestation of the signs of an electric current during the contraction of the muscles; thus in exciting contractions in one of my piles composed of muscular elements, in which the circuit was completed by the galvanometer, the signs of the muscular current were never perceived to increase. Finally, I have shown that the induced contraction is propagated through a coating of turpentine, which is of a nature sufficiently insulating to arrest the passage of any electric current.

I was therefore warranted in deducing from these phenomena that in the muscles which contracted, and so produced the induced contractions, there was never any electric current generated, and that therefore the induced contraction could not be explained by a reference to any such agency.

I will now cite some new researches instituted with a view to the discovery of the nature of the phenomenon of induced contractions, which is so obscure and at the same time so important.

Observing that the slightest discharges from the jar, inappreciable by the most delicate of our electroscopes, are invariably sufficient to excite violent contractions in the frog, it appeared agreeable to analogy to suppose that the cause of induced contractions might reside in a discharge similar to that of the jar, taking place in the muscle in the act of contracting. If that had been the case, it would no longer have been a matter of surprise that the galvanometer should give no indication during muscular contraction.

I began my researches by ascertaining whether, by passing very slight discharges of the jar through the muscular masses, contraction was excited in the galvanoscopic frog, the nerve of which touched the muscle traversed by the discharge. In effect, the galvanoscopic frog never fails to contract under the influence of extremely feeble discharge, such as are elicited after a very small jar has been discharged two or three

times with a metallic arc. It is needless to say that the galvanoscopic frog remains insulated in this experiment, merely having its nerve resting upon the muscle.

In like manner it must be observed, that in the above experiment muscles were employed which had ceased to contract during the discharge, so as not to have any induced contraction.

It must then be admitted that, in spite of the good conductibility of a muscular mass, a part of the discharge always escapes to the surface of the muscle and so traverses the nerve of the galvanoscopic frog.

The occurrence of this phenomenon is still more remarkable when the nerve of the galvanoscopic frog is placed upon a metallic surface through which the shock is passed.

The phenomenon may very well be produced by passing the discharge through a plate of tin or gold upon which the galvanoscopic nerve is laid out; only in this case we have not so many successive shocks producing contraction as with the muscular arc; evidently because the jar is much more perfectly discharged through a metallic conductor than through the muscle.

The next step therefore to be verified was whether a very slight discharge passed through a muscular mass would still excite the galvanoscopic frog to contraction, even when an insulating medium was interposed between the surface of the muscle and the galvanoscopic nerve. It also suggests itself naturally to the experimenter to employ in every experiment the same insulating coatings which are known to destroy induced contractions.

In this view I have covered the muscular mass with a coating of turpentine, and laid the galvanoscopic nerve upon it. On passing very slight discharges, the galvanoscopic frog invariably contracts. I have repeated this experiment very frequently, and the only difference that I have remarked has been in the number of successive shocks acting through the turpentine. The shocks are generally fewer than when there is no turpentine. Thus with a very small jar, and without the turpentine, five or six, and even as many as ten successive shocks may be obtained, and which cause the contraction; and when the turpentine has been interposed, the shocks are from four to six. The depth of the insulating stratum was always such that a current from a pile of fifteen couples of plates could not penetrate it.

I have gilded the muscular conductor, and stretched the galvanoscopic nerve upon its surface; on passing shocks from the small jar I still had contractions in the galvanoscopic frog. In this case also it is to be observed that contraction never went beyond the second or third discharge.

Finally, on interposing very fine plates of mica between the surface of the muscle and the galvanoscopic nerve, the frog contracted at two or three successive discharges from the small jar. I will here call attention to a fact somewhat remarkable, which has always occurred in these experiments: on passing the first shock of the jar accompanied by the spark, the contraction of the galvanoscopic frog almost invariably fails.

What inferences may be drawn from these experiments to aid our decision as to whether the induced contraction admit of an explanation on the supposition of an electric discharge similar to that of the jar, and taking place during the contraction of the muscle?

Before examining this point, I will relate a few other experiments upon the induced contraction which throw some light upon the interpretation of this phenomenon.

In a living dog, the spinal marrow (deprived of its membranes), the brain, the sciatic nerve, and the muscles of the thigh were all laid bare. Galvanoscopic frogs were placed in the usual manner upon these several parts, and at the same time the animal was irritated either by squeezing his paw, or by wounding the coverings of the spinal cord. The muscles of the thigh were thrown into violent contraction, and the animal howled from pain. In the meantime the galvanoscopic frogs placed upon the muscles in contraction alone exhibited the phenomenon of induced contraction. The same experiment was instituted upon a rabbit, and was followed with the same results. In like manner galvanoscopic frogs were arranged upon the abdominal viscera of a living rabbit while an electric current was passed along the pneumogastric nerves, or through the solar plexus. Never in the above experiments did the galvanoscopic frogs exhibit any indication of induced contractions. Now if it be borne in mind that in the experiments just related, while the animal shrieked with pain and was convulsed, there certainly was a current of that force, whatever may be its nature, which we term nervous, and yet no contraction in the galvanoscopic frogs placed on the nervous structures, we are forced to conclude *that the phenomenon of induced contraction belongs exclusively to the muscle in the state of contraction.*

With regard to the question whether the induced contractions admit of being explained by the supposition of an electric discharge elicited in the act of the contraction of the muscle, we are obliged to confess that the experiments recorded in the commencement of this memoir (and which are the only ones that in the present state of the science could have been instituted with a view to the decision of the question) are insufficient to afford a satisfactory solution.

Admitting that an electric shock analogous to a very slight discharge of the jar did actually take place during the contraction of the muscle, we should have no other means of ascertaining its existence than that of the induced contractions, the origin of which we seek to discover.

In effect, not one of our electroscopic instruments is capable of disclosing to us the existence of an electric discharge, such as that given by a very small jar previously discharged three or four times successively with a metallic conductor.

The galvanoscopic frog alone can indicate the existence of these discharges.

Respecting the influence of the coating of turpentine upon the two phenomena under consideration, induced contractions, and discharge through a muscle which acts likewise upon the galvanoscopic frog, we have seen that no difference existed between the two cases.

A difference, however, does exist on interposing a lamella of gold or of mica between the galvanoscopic nerve and the muscle: these lamellæ prevent the induced contractions without destroying the effect of the discharge of the jar. I am far from concluding from the above fact that the two phenomena may not have the same origin. We have no precise knowledge as to why the discharge of the jar can be very slight without its ceasing to excite contraction in the galvanoscopic frog; and we have seen in those cases in which the lamellæ of mica or of gold were interposed, that the number of successive discharges from the jar, which acted upon the galvanoscopic frog, was always less than when no lamellæ were interposed.

We must therefore pause a little before we can establish the fact of induced contractions not being due to an electric discharge produced during the contraction of the muscle; and since it is impossible for us to solve the question by direct experiment, let us be guided by such analogies as appear to have the best possible foundation. I purpose examining this field of investigation in a general summary of all my electro-physiological researches, which summary I hope soon to be able to complete.

PART II.

Upon the Phenomena elicited by the passage of the Current through the Nerves of a living Animal, or an Animal recently killed, according to the direction of the Current.

In my fourth memoir I took great pains to prove at length by the aid of experiment, that the electric current transmitted along a nerve modifies the excitability of the nerve in a manner differing widely according to the direction of the current; thus the direct current rapidly exhausts this excitability, while the inverse current increases it. Starting from this fact, I hope that I have given a satisfactory theory of electro-physiological phenomena.

Among the different experiments described on this head, I indicated one in particular which appears very singular, and which I have since studied in all its bearings. The frog prepared in the ordinary manner, and divided in the pelvis, is placed astride between two little glasses in which the reophores of a Faraday's pile of fifteen or twenty elements were immersed. It is evident that one of the limbs is traversed by a direct current and the other by an inverse current. It is unnecessary to describe here minutely the phases of the phenomena which present themselves during twenty or twenty-five minutes.

In the first place, the two limbs contract both on closing and on opening the circuit, after which there is contraction of the limb traversed by the direct current on closing the circuit; and the other limb contracts on breaking the circuit: finally, only one limb contracts, viz. that of the inverse limb on the cessation of the passage of the current. On keeping the circuit closed for some minutes, we invariably remark that the inverse limb, which contracts on breaking the circle, is seized with a permanent contraction of a decidedly tetanic character. This phenomenon is of importance,

as it indicates an intimate connection between nervous influence and the action of the electric current according to the direction of the latter.

I have therefore exercised the greatest care in studying this phenomenon; and if I have not been able to show the connection in all its evidence, and to express it with that degree of simplicity which is the characteristic of physical laws, I venture to hope that this failure will in part be attributed to the obscurity in which the subject is involved, and that some degree of favour may be accorded to the efforts which I have made in this direction. It is a very rare circumstance to find a frog that does not present the phenomenon which has been already described; and in particular those frogs which pass several days in winter without nourishment, never fail to manifest it. In this case they appear more disposed to become tetanic; and, in effect, they do almost all become so when they are prepared by dividing their spinal marrow, and remain so during some seconds. The phenomenon in general manifests itself after the current has been passed for twenty-five or thirty minutes. The tetanic contraction lasts a very long time, and it often happens that when this has ceased, there are twitchings in the limb from time to time. These phenomena equally occur on passing the current through the nerves without its traversing the muscles. In like manner they may be elicited in the living frog, only in this case the tetanic contraction lasts a much shorter time. The phenomenon never occurs when the current acts only on the muscle, which it may be made to do by disposing the frog in the ordinary manner, without having divided the pelvis. On the other hand, it presents itself after the spinal marrow has been destroyed.

It is not essential to the manifestation of the phenomenon in question that the muscles should be thrown into contraction at the commencement of the passage of the current, which may easily be seen by closing the circuit by the aid of a bent metallic conductor ending in some paper which *slowly* imbibes a certain quantity of water. If, instead of arresting the current by removing one of the reophores of the pile, a metallic arc be introduced between the two glasses, the tetanic contraction occurs just the same. But if to the end of this curved rod be attached some paper, so as to occasion the passage of the current to cease more slowly as regards the frog, then instead of one tetanic convulsion we have a series of contractions succeeding each other at short intervals of time.

In whatever manner the current through the nerve of the inverse limb is arrested, the tetanic contraction is excited. It suffices for this to moisten the nerve with a large drop of water, or to double it back upon itself for the contraction to take place; while this does not occur if the current is arrested for the muscle. This is easily effected by bringing the reophore of the pile, while the circuit is still kept closed, in contact with the thigh at the point where the nerves immerge.

The following experiment proves still more clearly the part which the nerve plays in this phenomenon. If, while the circle remains closed, and it has been previously ascertained that the tetanic contraction will follow the opening of the circuit, the

nerve be divided rapidly at the precise point where it enters the muscle, the limb is thrown into contraction without its remaining in a state of tetanus. But if, instead of this, the nerve be divided higher up near to its issue from the spinal marrow, then the tetanic contraction takes place as usual.

We will next consider the circumstances which modify or destroy this phenomenon. None of those frogs which were killed after the administration of large doses of morphine, so as to manifest every symptom of narcotism, ever exhibited tetanic contractions. The same thing occurs with frogs which have been made to support heavy weights with their legs. The circumstance which suddenly puts a stop to the tetanic contractions is the passage of the inverse current, the same, that is to say, by which the phenomenon is produced. Generally two or three seconds after the circle is closed again, the limb falls into its natural posture. If, on the other hand, the current merely ceases to pass, it will in general be three minutes before that happens; and the same is the case pretty nearly if the direct current is passed. To obtain these phenomena with great precision, care must be taken not to reverse the direction of the current upon the same nerve several times successively.

I was desirous of trying whether the species of tetanic contraction, or rather of corpse-like rigidity with which a muscle is seized after a shock from the jar, was dissipated by the passage of the current, direct or inverse, through its nerve. This is not at all the case, and whenever it has occurred, the passage of the current has not produced contraction.

I will here state that, acting with the electric current upon the nerves of living warm-blooded animals, as rabbits and dogs, a phenomenon analogous to that which we have studied in the frog is very clearly seen to follow the action of the inverse current; only it is observable that in these animals the tetanic contractions last a much shorter time, especially if the vitality of the animal is very great; the same fact besides is verified in the frog.

How is this tetanic action produced? It is easy to convince oneself, if any doubt could be entertained upon the subject, that there is no electricity rendered latent either in the nerves or in the muscles by the passage of the inverse current. My endeavours to discover signs of any, by the aid of the condenser, have been entirely fruitless. Likewise there are no signs on opening the circuit of any electric current in circulation. I have made myself quite certain of this fact by means of the galvanometer, employing at the same time a pile of tetanized frogs.

The phenomenon which we have studied thus at length belongs clearly to the nerve, and takes its origin from the relation, the nature of which is yet undetermined, which exists between nervous influence and the action of the electric current according to the direction of the latter.

It has been distinctly proved in the fourth series of these electro-physiological researches, that the passage of the direct current destroys the excitability of the nerve, and that this is not the case with the inverse current, which acts in a contrary

manner. The direct current acts by exciting, when it commences, a contraction which we know to be stronger than that produced in the same case by the inverse current. A limb traversed by the direct current may be compared to a limb fatigued by repeated efforts. The inverse current may be supposed to act in an opposite manner, and during its passage the nervous force might accumulate in the nerve.

I prefer, however, suppressing considerations of this nature on account of the vague and uncertain impression which they are calculated to leave in a reflecting mind.

It may not, however, be without some utility to close my electro-physiological researches with an attempt at embracing, under some general views, the phenomena of muscular contraction, of the production of electricity in fish, and of the relation between the electric current and nervous influence.

I would here recall the facts discovered by M. Longet and myself, on treating the motor nerves with the electric current. Monsieur Longet and myself discovered that the action of the electric current upon these nerves was precisely the opposite of its action upon the mixed nerves.

The direct current acting upon the motor nerves, determines the contraction in the second period of the excitability of these nerves when it ceases to pass, and the inverse current produces it when it begins to pass. It is natural to think that the phenomenon of tetanic contraction would be produced by the cessation of the direct current when the motor nerves are acted upon. In connexion with this subject, I will here relate a phenomenon which I have lately discovered, and which appears to me to be worthy of remark. With the aid of a wheel armed with insulating metallic teeth, similar to that of Mons. Masson, I passed a current in a frog prepared in the manner described above, that is to say, like that placed astride between two glasses. In this manner I pass the current 500 or 600 times in the frog. After this, on attempting to interrupt, and then again establish the circuit, the following phenomenon strikes the experimenter. The inverse limb contracts on closing the circle, and the direct limb on opening the circle. On leaving the circle closed for some minutes, the ordinary phenomena—the opposite of the preceding—appear; that is to say, the direct limb contracts on closing, and the inverse limb on breaking the circle. It is very difficult to reproduce these phenomena in the same frogs, on account of the extreme feebleness produced by the numerous contractions.

The above fact establishes a fresh connection between nervous influence and the passage of the electric current according to the direction of the latter. I will return to this subject in the general views which I announced above.

Pisa, December 1846.

XIV. *Electro-Physiological Researches.—Sixth Series. Laws of the Electric Discharge of the Torpedo and other Electric Fishes—Theory of the production of Electricity in these animals. By Signor* Carlo Matteucci, *Professor in the University of Pisa, &c. &c. Communicated by* Michael Faraday, *Esq., F.R.S., &c. &c.*

Received May 20,—Read June 10, 1847.

The present memoir is not a mere description of a certain number of facts lately discovered on the electricity of electric fishes; besides this, and more than this, it contains the laws and theory of these phenomena. Consequently the order of exposition of the facts in this memoir will be the same as that which a scientific arrangement of the subject would dictate.

It is needless to remind the reader that the discharge of the electric fishes is subject to the will of the animal.

On irritating any point of the body of an electric fish, it is easy to demonstrate by experiment that this irritation is transmitted by the nerves to the fourth lobe of the brain, and that then only the discharge takes place. If the spinal marrow be divided at any part of its length in a living torpedo, every kind of irritation below the point of section fails to produce any effect.

It is equally easy to prove that the nervous action by which the discharge is determined under the influence of the will, resides in the fourth, or *electric lobe* of the brain; in effect, after the three superior cerebral lobes have been extracted, the torpedo can still give the shock either voluntarily, or from external irritations.

We also know that if one of the electric organs of a living torpedo be rapidly detached, we can still obtain the discharge on irritating one of the nerves ramified within the organ.

We have also shown, some time ago, that on acting upon these nerves with the electric current, we obtain the discharge under the same conditions and the same laws as those under which we have muscular contraction on acting upon the mixed nerves. Hence the remarkable analogy found to exist between muscular contraction and the electric discharge of fishes.

The most singular and the most important fact in a theoretical point of view is the discharge which we obtain when we take a very small part of a prism of the electric organ of the torpedo, and irritate it in any manner. Electric discharges always take place, as is shown by the contractions of the galvanoscopic frog.

I have very lately repeated this experiment in a variety of ways, and have always found that, on irritating one of the small nervous filaments distributed to the organ,

we obtain the discharge from that small portion of the organ into which the nerve penetrates. We have thus every reason to conclude that the electric organ of the torpedo and of all the electric fishes is composed of a great number of elementary organs, and that the elementary organ is nothing else but a nervous fibril in contact with a small cell filled with albumen. And since this cell gives an electric shock when it is subjected to nervous action, we are compelled to admit that under nervous influence the two opposite electricities separate to be instantaneously re-united.

This relation between nervous influence and electricity is, without doubt, of the same nature as that which exists between heat and electricity, between the electric current and magnetism. It is in studying the production of electricity in the different electric fishes, together with the distribution of nervous filaments in their electric organs, that we arrive at a better understanding of this relation between nervous influence (la force nerveuse) and electricity. Thus we see in the torpedo and gymnotus—the two electric fishes best known physically and anatomically—that the nervous filament always ramifies in the electric organs of these fishes perpendicularly to the axis of the prisms of these organs. Besides which we know that the extremities or poles of the electric organs in these two fishes are situated at the extremities of the prisms: in effect, in the torpedo these poles are the ventral and dorsal surfaces, while in the gymnotus the poles are at the head and tail of the animal.

It will be seen from this, that in this action of the nervous force, as exercised in the electric organs of these fishes, it follows the same law in developing electricity as does the electric current upon magnetic bodies. In effect, each prism of these electric organs cannot be considered otherwise than as a pile of elementary organs, upon each of which a nervous filament is spread normally to the axis of this pile. Now a cylinder of cast iron enclosed in a helix of metallic wire, and traversed by the electric current, is evidently an apparatus analogous to a prism of the electric organ of the fish at the moment when the nervous influence excites the discharge.

On one side we have the two opposite states of electricity, the tension of both of which must increase at the extremities of each prism in proportion to the number of elementary organs of which it is composed; on the other hand, we have opposite magnetic states, the strength of which at the poles is also proportional to the number of magnetic elements of the cylinder.

Experience has in fact shown, both in the torpedo and in the gymnotus, that the strength of the current obtained during the discharge is proportional to the length of the prisms included in the circuit closed. I have frequently seen, on dividing the electric organ of the living torpedo in a plane parallel to the surface of the organ, that the sum of the currents given by the different slices of the organ were approximatively equal to the current given by the entire organ. These same facts are verified also by including different points of the electric organ of the torpedo in the circuit; we know, in effect, that the current is stronger when points near the median line of the animal are touched, than when more remote points are. Nothing is easier than

to verify upon the gymnotus the fact that the strongest discharge is that obtained on including the entire length of the animal within the circuit.

In comparing the discharge of the torpedo with that of the gymnotus, so far as it is possible to do so, it is somewhat surprising to see that the two discharges are not so far different as they should be to be in proportion to the length of the prisms in the two animals.

But anatomy furnishes us with the key to this apparent anomaly, and shows us that the length of the prisms in the torpedo may be considered equal to that of the gymnotus, inasmuch as the number of elementary organs in the former is at least ten times as great as in the latter.

As regards the constancy in the direction of the current in the discharge of the electric fish, it results necessarily from the unvarying direction in which nervous influence is propagated through the nerves of the electric organs.

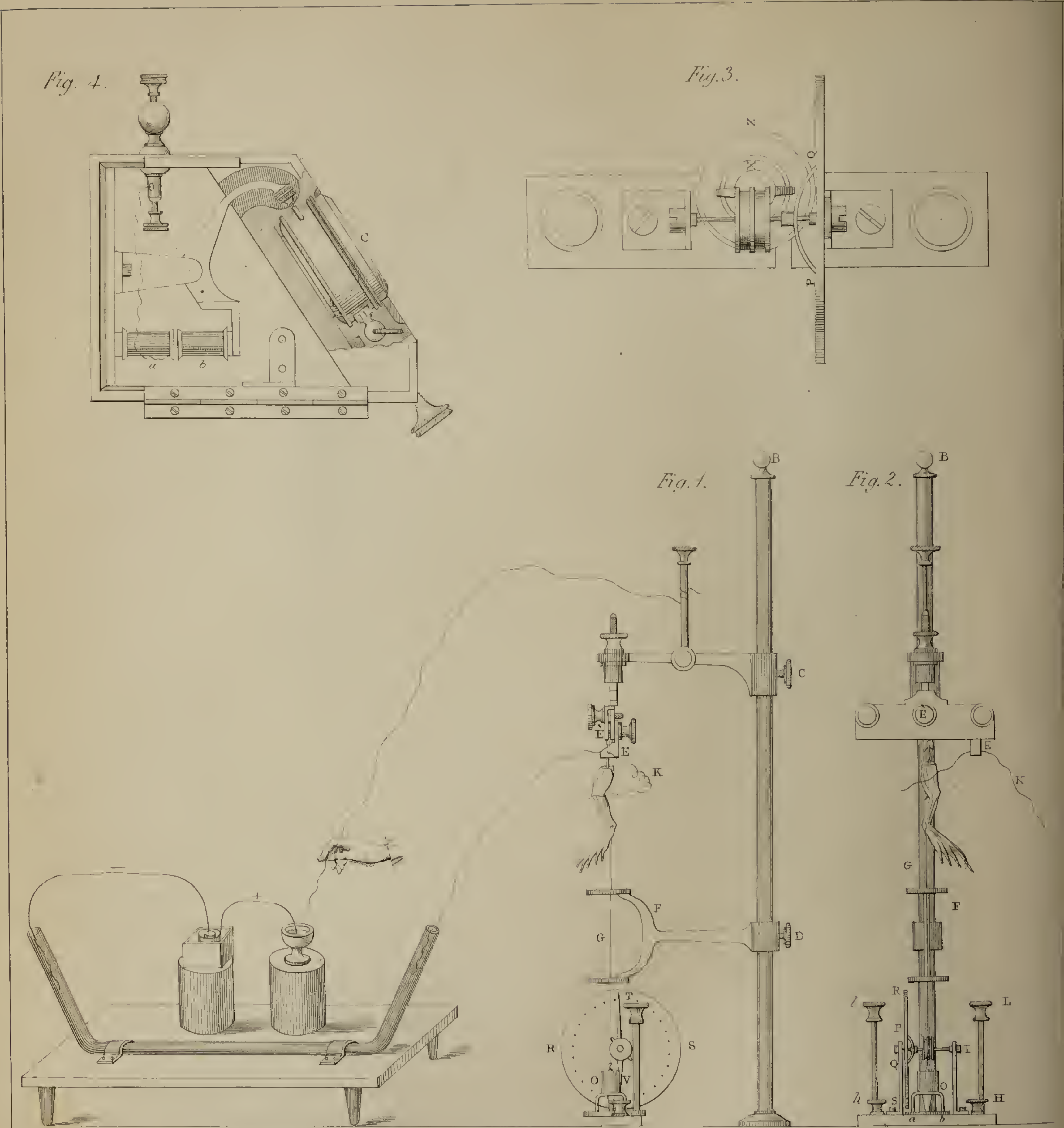
Experience has clearly shown that these nerves are charged with the sole function of exciting the discharge, so that the nervous action must constantly be propagated in a direction from the brain to the nervous extremities; the direction in which the separation of the two opposite states of electrical excitement ought to take place in the electric organ, must also necessarily be constant.

Finally, it results from all that has been said above, and it is proved by a vast number of experiments, that there exists between the nervous force and the electric states developed in the electric organs of fishes, that same relation or degree of intensity which always exists between two phenomena of which one is the cause of the other, such as exists between the electric current and the magnetism which it gives rise to.

In effect, without stopping to insist too much on what may be but vague in the physiological data, I cannot but admit that the nervous force increases independently of the will, with every increase in the activity of the functions of circulation and of respiration, and of every act of nutrition, as also under the influence of certain agents introduced into the organism. A great many experiments have fully convinced me that the electric shock of the torpedo increases with these same vital actions. Thus the torpedo in water, a little above the ordinary temperature, produces stronger discharges; the effect is precisely opposite if the respiration or the circulation of the blood of these fishes be hindered. The torpedo and the gymnotus give discharges decreasing in strength in proportion to the number given, and they reacquire their faculty of giving more powerful shocks after an interval of repose. The torpedo, over-excited by nux vomica, gives shocks more powerful than usual. This last and the preceding facts establish the connection between the intensity of the nervous force and that of the electric discharge of fishes.

I trust that I have demonstrated in this memoir, which contains the summary of my numerous researches upon the phenomena of electric fishes, that there exists in the electric organs of these fish a very simple case of relation between nervous force and electricity, established by well-determined laws.

Pisa, January 1847.



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XV. *Electro-Physiological Researches.—Seventh and last Series. Upon the relation between the intensity of the Electric Current, and that of the corresponding physiological effect. By Signor* Carlo Matteucci, *Professor in the University of Pisa, &c. &c. Communicated by* Michael Faraday, *Esq., F.R.S., &c. &c.*

Received May 20,—Read June 10, 1847.

In bringing to a conclusion my series of researches on electro-physiology, I shall dwell in the present memoir on such of them as have frequently engaged my attention, and which I have lately again enlarged upon as bearing directly upon the highest, and I would even say, most physical point of the science of electro-physiology.

We admit as clearly demonstrated by experiment that electro-magnetic, as well as electro-chemical action, give the measure of the electric current; in other words, that different quantities of electricity produce chemical and magnetic effects proportional to these quantities. What then is the nature of this relation between the quantity of electricity and the contraction thereby excited, when transmitted through the nerve of an animal either living or killed as recently as possible? In an early memoir, published three years ago in the Ann. de Chem. et de Phys., and in a communication which I had the honour of making to the British Association at York, I described my first experiments on this subject. Nevertheless I have always been desirous of being able to renew these experiments with far more perfect instruments than I possessed at that time.

All the apparatus which I have employed were executed by M. Breguet with his accustomed skill and talent. The principal among them (Plate XII. fig. 1) has been already described in my Fourth Series. In the course of the present memoir I will give the description of the other instruments.

The following is the general disposition of the experiment. A frog prepared rapidly is reduced to a thigh with the leg, the lumbar nerve, and a morsel of spinal marrow. An electric current, or a discharge from the Leyden jar, is passed through a certain length of this nerve. The electro-physiological effect is the contraction of the limb, which at the expiration of a given time is raised to a certain height. Let a determined quantity of electricity now be passed, then the half, a third, a fourth of that quantity, and so on, and a measure be taken of the corresponding electro-physiological effect; that is to say, of the height to which the limb is raised, and within what time.

It will be seen from this that there is no difficulty in putting forward the subject of the research, but the practical difficulty is very great. It is needless to describe

in what this difficulty consists, it being sufficiently apparent in itself. I have therefore confined my efforts to the discovery of the relation between the electro-physiological effects, and the quantities of electricity which produce them, finding it almost impossible to arrive at approximative measures of these absolute effects.

On passing an electric current from different piles along the nerve of a frog disposed in the apparatus described in my Fourth Series, fig. 1, and noting the movements of the index in the different cases, it will be perceived that the limb is raised to the same height for very different currents. Thus I have found that a current from a Grove's pile of six couples of plates, and one from six of a Faraday's pile with and without a powerful magnet in the circuit, and finally a current from only one couple of Wheatstone's plates, give the same number of degrees for the movement of the index of my apparatus. It is clear from the above that all these currents are too strong for the effects to be compared together; it would be precisely the same case if we were to pretend to measure different currents with a galvanometer the needle of which is propelled to 90° by the most feeble of these currents. The real object, therefore, to be ascertained was the feeblest current which produced the greatest possible medium contraction. I succeeded in the following manner in discovering this. I employed a small Wheatstone's pair of plates, of constant force, introducing within the circuit a cylinder of distilled water. This water is contained in a glass tube a centimètre in diameter, and bent in the form of the letter U. By plunging the metallic conducting wires more or less deeply in this tube, I vary at will the resistance of the circuit. The following are all the details of the experiment. The Wheatstone pair of plates was exactly like that described in the memoir of that skilful experimenter; the circuit is composed of a copper wire covered with silk, and half a millimètre in diameter, which serves to close the circuit interrupted by the cylinder of distilled water. The extremities of the wire which dips in the cylinder were of platinum; the length of the copper wire in all about three mètres. The circuit was closed and broken by rapidly plunging one end of the wire, held in the hand, into a capsule of mercury. Both the pile and my apparatus were perfectly insulated, and the copper wire which served to close and break the circuit was varnished at that part which was held in the hand. The depth to which the current, which I call limited, penetrated within the column of water was about eighty-eight centimètres (in the case of very vivacious frogs rapidly prepared) during the first fifteen or twenty contractions. In order to render these experiments as comparable one with another as is possible in this kind of research, it is necessary that the circuit be broken always at equal intervals of time. I leave the circuit closed as short a time as possible, and invariably allow from fifteen to twenty seconds between one experiment and the other. I always employ the direct current for reasons well-known.

The difference occasioned by the column of eighty-eight centimètres of water, and which showed that I had arrived at the limited current, was very manifest in my apparatus; thus at a depth of a few centimètres less I had the highest indication, the

same as that given when the stratum of water was not included in the circuit. With the column of water eighty-eight centimètres in depth, the indication was lessened by from 2° to 4°, and with a column of from ninety to ninety-five centimètres the diminution was very sensible.

In the very numerous experiments which I have instituted on this subject, I have always, in each in particular, sought for the length of the stratum of water which gave the limited current. I repeat that it was very easy to effect this by approximating the wires more or less in the water. It remains now to be considered by what means the half, a third, a fourth of the quantity of electricity was made to pass into the same nerve. After several trials I returned to my first plan as being the least inexact possible. When I wish to pass half the quantity of electricity through the nerve, I interpose a second nerve between the forceps and the thigh; to pass a third of the current I add two nerves, and so on. Theoretically, and with regard to the great resistance of the circuit, it may be admitted as true that the half of the current passes into each of the two nerves, and a third into each of the three nerves, the same ratio being observable according as the subdivision of the current proceeds; but having respect also to the bad conductibility of the nerve, which is certainly, from its nature* and dimensions, at least as great as that of the stratum of water, it might be imagined that in increasing the number of nerves the total current would be also augmented, and by that means rather more than the half of the current which passed into one nerve would pass in the case of the two nerves; in the case of the three nerves, somewhat more than a third of the current for each nerve; and so on. By introducing a galvanometer within the circuit, we are able to ascertain the very slight augmentation produced by the nerves successively added†.

In order that these nerves might be similar in all points, as nearly as possible, they were always taken from frogs prepared at the same time as the frog which served for the experiment. Care must be taken in placing them side by side not to allow them to touch one another. I have invariably observed that when this happened, and that the nerve of the frog within the apparatus was enveloped by the nerves added subsequently, there were no longer any signs of contraction on passing the current, though these were immediately manifest when the same number of nerves were prevented from contact with one another. I look upon this result as a further proof that the cause of contraction from the passage of the electric current, is the discharge which takes place at the commencement and at the termination of the circuit.

The following is the march of the phenomena in these experiments. If we begin to add the second and third, &c. nerve while the current is still tolerably far from

* A hempen thread soaked in water, and a nervous filament as nearly as possible of the same dimensions, offer degrees of resistance which are in the following ratio, twelve for the moist thread, and fifteen for the nerve. The nerve must be very fresh to afford this resistance: it naturally increases in proportion as it becomes dry.

† In performing these experiments, I have found that if the stratum of water was more resisting than the nerve, there was no perceptible increase in the current from adding two, three, or four nerves to the circuit instead of one. In my experiment the resistance of the column of water is greater than of the nerve.

its limit, we invariably observe that the contraction remains the same, or nearly so, as when there was no other nerve than that of the frog contained in the apparatus. On the contrary, if these experiments were not performed until the current was less than the limited current, then, after the second nerve had been added, there was no longer any contraction that could be measured.

It is, I repeat, by taking our departure from the limited current that we must determine in every case, performing the experiments with all the precautions described, and always being careful to wait until the natural contractions of the frog have ceased; and in this manner we may attain to numerical results which will admit of being compared together.

Before stating these numbers I must speak of a research which it was necessary to make in order to render these results as exact as possible. I allude to the duration of these contractions, both great and slight. I have measured these with the apparatus (figs. 2, 3, 4), which appears to me analogous in principle to that upon which the instruments invented by Messrs. WHEATSTONE and BREGUET for measuring the velocity of projectiles are founded.

This is a magnet (*a*, fig. 4), the anchor (*b*) of which is moveable, and at each movement strikes against the knob of a chronometer (*c*). The small weight (*o*, figs. 2, 3) which the frog raises in contracting is constructed in such a manner as to complete or break the circuit of the magnet. When the contraction takes place the circuit is broken, and it is re-established an instant afterwards when the contraction ceases. It then is broken again, and so continues to alternate. It will be seen from this that the interval of time elapsed between two successive contractions executed without any time being lost between them, will be marked by the chronometer.

I have found that in this manner the interval is constant for the first ten or fifteen seconds, during which it is $0''\cdot25$; after which it becomes longer, and remains $0''\cdot33$ for ten or fifteen seconds longer, and afterwards it becomes $0''\cdot41$, $0''\cdot58$, &c.

By varying the length of the stratum of water in three different experiments and with the frog still vivacious, my apparatus showed contractions differing from 6° to 10° up to 28° or 32°. The duration of the interval was perceptibly the same in all ($0''\cdot30$ to $0''\cdot33$).

Finally, it remained to measure the duration of the two different acts which are produced in the frog's limb in the interval between one contraction and the next, that is to say the limb is raised and contracts, then ceases to contract and falls.

I employed to ascertain this the same method which, I believe, the celebrated WATT adopted in the first instance for determining the velocity of the pistons in his machines. A fine point was attached to the little shank fastened to the leg of the frog, which point scraped, during the contraction, against a rapidly revolving smoked disc, of which the rotations were perfectly uniform.

The trace which the point leaves on the disc during its elevation may serve to indicate the duration of this elevation when it is known how long it takes to perform one revolution of the disc. The disc I used performed forty-eight revolutions in $1''$.

I have found from a great number of experiments, that the duration of the true contraction was much shorter than that of the descent or return of the limb, and that the difference became so much the greater as the muscle became fatigued.

I estimate the real contraction in my experiments at less than $\frac{1}{100}$th of a second. This duration was perceptibly the same for both great and slight contractions.

The following table shows the numbers found in a great variety of experiments conducted in the manner described, the number of nerves being varied.

I will only give a certain number of my experiments without making any selection therefrom.

	Number of nerves.	Degrees of contraction.		
First experiment	3	6	8	8
	2	10	10	10
	1	24	22	14
Second experiment	3	11		
	2	22		
	1	30		
Third experiment	2	12	14	
	1	26	28	
Fourth experiment	3	8	6	
	2	14	16	
	1	26	26	
Fifth experiment	3	6		
	2	14		
	1	24		
Sixth experiment	2	20	16	
	1	32	28	
Seventh experiment	1	6	6	
	2	3	4	
Eighth experiment	2	12		
	1	20		
Ninth experiment	2	6		
	1	10		
Tenth experiment	3	3		
	2	10		
	1	20		

Examining attentively the numbers which I have just exposed, and taking into consideration the reflections made above, and those principally which establish the fact, that the total current increases perceptibly with the number of nerves, it appears to me that we ought to conclude from these experiments that it is sufficiently demonstrated, and true as far as this kind of experiment admits of demonstration, that the electro-physiological effect is proportional to the intensity of the current.

Pisa, February 1847.

Postscript.

Received May 20th, 1847.

Extract from a Letter from Professor Matteucci to Mr. Bowman, dated Pisa.

(*Translation.*)

"As I do not propose to resume my electro-physiological researches, at least for some time to come, I should feel obliged by your adding the following fact, which is a very striking one, to the memoirs just presented to the Royal Society.

"You know the law of the electro-physiological action of the current on mixed nerves: you know that this law is very different in the case of the simple nerves of the anterior roots, as I found in conjunction with Longet.

"Now, if you render a rabbit or a dog insensible by the inspiration of sulphuric ether, and, while in that state, pass the direct current along one sciatic nerve, and the inverse along the other, you will have the following phenomena:—

"1st. If the animal is not *totally* insensible, some cries of pain at the commencement of the *direct* current, which continue more or less while it is passing, but *no contraction*: the contraction with this current only appears on *interrupting* the current.

"2nd. With the *inverse* current, no cry or sign of pain on completing the circuit, if the animal is thoroughly etherized, and slight cries if not quite etherized: *contraction* of the muscles in *closing*, *none* on *opening* the circuit.

"These are the phenomena of the anterior roots.

"3rd. Now cut the nerves at their insertion into the spinal marrow: instantly the phenomena are reversed, and we have the ordinary play of the mixed nerves; that is to say, the *contraction* with the *direct* current takes place on *closing*, and that with the *inverse* current on *opening* the circuit.

"In the mixed nerves, therefore, the phenomena are complicated by the presence of the sensitive fibres.

"This conclusion appears to me very important, and I much regret that I am unable to tell you that the same things occur with the frog. Etherized frogs give the ordinary phenomena of mixed nerves, as heretofore.

"But who knows how the two kinds of fibres mingle in the nerves of different animals? Who is so ignorant as to believe that, in such matters, he can know all, and all at once?"

XVI. *On the Value in Absolute Alcohol of Spirits of different Specific Gravities.*

By GEORGE FOWNES, *Esq.*, *F.R.S.*,

Professor of Practical Chemistry in University College, London.

Received June 7,—Read June 17, 1847.

HAVING been for some months past occupied with experiments on the fermentation of sugar and molasses, and having found it necessary to construct for this purpose a new table of the quantity per centum by weight of absolute alcohol contained in spirits of different specific gravities, I venture to lay the same before the Royal Society, hoping that it may be found generally useful in inquiries of this kind, and also for other purposes.

The Table was formed synthetically; absolute alcohol and distilled water were weighed out in the required proportions, mixed in small well-stopped bottles and well-shaken together. After standing three or four days the mixtures were brought to the temperature of 60° FAHR. exactly, and their specific gravities determined with great care. After the lapse of two or three days more this last-named operation was repeated, but in no case was it observed that any further contraction had occurred. Neither was the specific gravity of a mixture containing nearly equal parts alcohol and water which had been so examined changed by being inclosed in a strong accurately-stoppered bottle, and heated for some time to a temperature above its boiling-point.

In this manner each alternate number in the Table (each even number) was obtained by direct experiment; the others were then interpolated. When completed, the Table was examined by various methods calculated to test its accuracy, but no error of sufficient magnitude to limit its usefulness was detected.

The absolute alcohol employed in these experiments was prepared in the following manner:—the strongest rectified spirit was agitated with half its weight of carbonate of potash deprived of water of crystallization, and left in contact with the salt some days. It was then decanted upon half its weight of powdered quicklime, made from black marble, contained in a metal still which could be perfectly closed. The mixture of spirit and lime was retained in a warm situation for a week or thereabouts, and then distilled by means of a water-bath. By this treatment the specific gravity of the alcohol was generally reduced to ·796 or even below, and by a repetition of the process of digestion with powdered lime and re-distillation, the last traces of water were removed. In this manner, without difficulty, the very considerable quantity of absolute alcohol required for the experiments was procured.

Absolute alcohol thus obtained has the specific gravity ·7938 at 60° FAHR.; it is extremely expansible by heat, which renders the determination of its exact specific gravity difficult and troublesome when the temperature of the room is either above or below 60°. The same remark applies to the mixtures of alcohol and water extending over more than half the Table, the most minute precautions regarding temperature being necessary to avoid serious errors. In a glass retort containing pieces of copper-foil absolute alcohol boils at 177° FAHR., the barometer standing at 29·75 inches. Lastly, when analysed by combustion with oxide of copper, it yields numbers representing the proportions of carbon and hydrogen present so closely agreeing with those required by theory, as to leave no doubt of its purity and freedom from all admixture.

The contraction of volume suffered by various mixtures of alcohol and water may be rendered obvious by comparing the actual specific gravities of such mixtures with the calculated mean specific gravities. In the accompanying Plate (XIII.), in which the vertical lines represent the per-centage of alcohol by weight, and the horizontal lines the specific gravities, the calculated mean specific gravities of the mixtures are seen to form a straight diagonal line from corner to corner, while the actual specific gravities form an irregular curve with upward convexity, rising quickly to near its maximum deviation at 30 per cent., running nearly parallel with the other line to 50 per cent., and thence declining until it reaches the extremity of the scale.

University College,
June 7th, 1847.

Comparison of mean and actual specific gravities of various mixtures of alcohol and water.

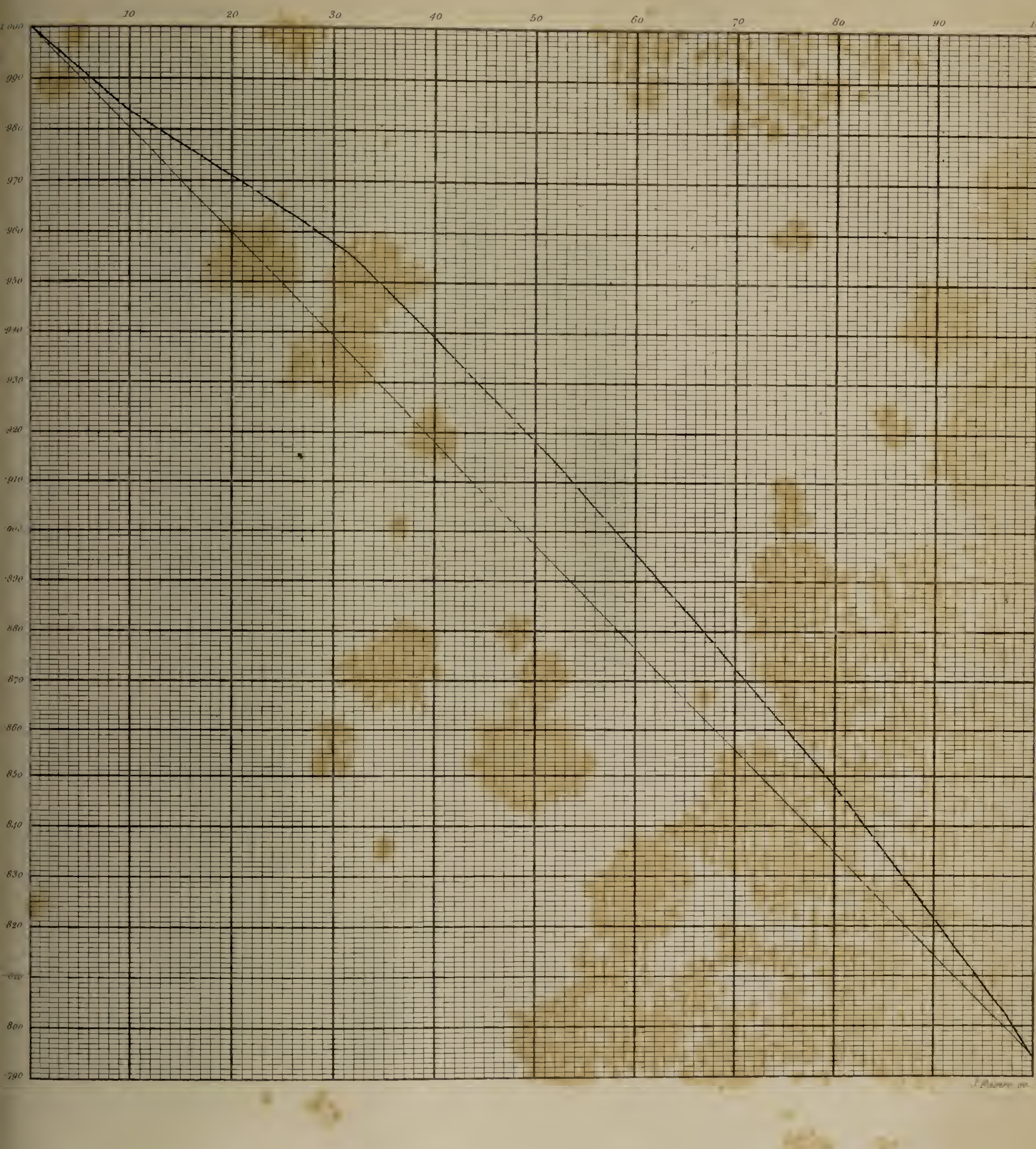

Table of the proportion by weight of real or absolute alcohol contained in 100 parts of spirits of different specific gravities at the temperature of 60° Fahr.

Specific gravity.	Per-centage of alcohol.	Specific gravity.	Per-centage of alcohol.	Specific gravity.	Per-centage of alcohol.
·9991	0·5	·9511	34	·8769	68
·9981	1	·9490	35	·8745	69
·9965	2	·9470	36	·8721	70
·9947	3	·9452	37	·8696	71
·9930	4	·9434	38	·8672	72
·9914	5	·9416	39	·8649	73
·9898	6	·9396	40	·8625	74
·9884	7	·9376	41	·8603	75
·9869	8	·9356	42	·8581	76
·9855	9	·9335	43	·8557	77
·9841	10	·9314	44	·8533	78
·9828	11	·9292	45	·8508	79
·9815	12	·9270	46	·8483	80
·9802	13	·9249	47	·8459	81
·9789	14	·9228	48	·8434	82
·9778	15	·9206	49	·8408	83
·9766	16	·9184	50	·8382	84
·9753	17	·9160	51	·8357	85
·9741	18	·9135	52	·8331	86
·9728	19	·9113	53	·8305	87
·9716	20	·9090	54	·8279	88
·9704	21	·9069	55	·8254	89
·9691	22	·9047	56	·8228	90
·9678	23	·9025	57	·8199	91
·9665	24	·9001	58	·8172	92
·9652	25	·8979	59	·8145	93
·9638	26	·8956	60	·8118	94
·9623	27	·8932	61	·8089	95
·9609	28	·8908	62	·8061	96
·9593	29	·8886	63	·8031	97
·9578	30	·8863	64	·8001	98
·9560	31	·8840	65	·7969	99
·9544	32	·8816	66	·7938	100
·9528	33	·8793	67		

XVII. *On different Properties of Solar Radiation producing or preventing a deposit of Mercury on Silver Plates coated with Iodine, or its compounds with Bromine or Chlorine, modified by Coloured Glass Media and the Vapours of the Atmosphere. By* A. Claudet, *Esq. Communicated by Sir* David Brewster, *F.R.S., &c. &c.*

Received June 10,—Read June 17, 1847.

FROM the commencement of photography it has been known that the red, orange, and yellow rays exert but a very feeble photogenic influence on the Daguerreotype plate. The experiments of several philosophers, especially those of Sir J. Herschel on photogenic papers, published in February 1840, prove that this action is more particularly confined to the most refrangible part of the prismatic spectrum, commencing from the space found covered by the blue rays and extending to the extremity of the violet, and sometimes even beyond it.

In 1839, Sir J. Herschel observed that the red rays exercised on several photogenic papers an antagonistic action to the photogenic rays, modifying their effect. Contrary to this, in 1841, M. Ed. Becquerel presented to the Paris Academy of Sciences a memoir, in which he announced that the red, orange and yellow rays were endowed with the property of continuing the action commenced by the photogenic rays; these latter he called *exciting rays*; to the first he gave the name of *continuing rays.*

M. Ed. Becquerel made his experiments on photogenic papers, and added that he had observed the same effects on the iodized silver plate.

Dr. Draper of New York published in the Philosophical Magazine for November 1842, some remarks on a class of rays which he supposed to exist in the light of the brilliant sun of Virginia, and which had the property, when separated, of entirely suspending the action of the diffused light from the sky; these antagonistic rays extended from the blue to the extremity of the red, and appeared to be almost as active in preventing the decomposition of the iodide of silver as the blue rays were in producing it.

In January 1845 a memoir was read by me at the Society of Arts, London, in a part of which I recommended opticians to construct object-glasses in which they should particularly correct the chromatic aberration of the long photogenic space of the solar spectrum, even at the cost of the achromatism of the less refrangible rays. This, however, had been already indicated, without my being aware of it at the time, by Sir J. Herschel; but I added that the greater separation of the visual and photogenic focus which might result from such a combination, according to the quality

of the glass employed, would be an advantage, by dispersing, at the focus or on the plate, beyond the photogenic lines, the red, orange or yellow rays; for the reason, that if they were brought to the same point they would tend to neutralize and destroy the effect of the photogenic rays.

In October 1846, M. Lerebours announced to the Paris Academy of Sciences that the red rays prevented the action of the photogenic rays; this announcement induced Messrs. Foucault and Fizeau to publish immediately similar results, which they had previously consigned to the Academy in a sealed memoir, bearing date May 1846.

These communications of Messrs. Lerebours, Foucault and Fizeau, led Dr. Draper to write a letter, published in the Philosophical Magazine of February last, repeating his observations on the spectrum of Virginia, adding several other analogous facts confirming the theory of a protecting and even destroying action exercised by the least refrangible rays. Dr. Draper, in the same letter, said that the rays which protect the plate from ordinary photogenic action are themselves capable, when isolated, of producing a peculiar photogenic effect.

Soon after the publication of M. Ed. Becquerel's memoir, M. Gaudin made some analogous researches on the Daguerreotype plate; and he succeeded in developing an image as perfect as that produced by mercury, by submitting the plate, when taken from the camera obscura, to the action of light alone under a yellow glass, and without any subsequent exposure to mercury.

This curious discovery gave some hope that, from the supposed continuing action of the red and yellow glasses, by submitting the plate alternately, or simultaneously, to the action of the mercury and of these glasses, an accelerated development of the image would result; but all the researches made to arrive at this point have been fruitless; and, until the present time, the labours of Messrs. Becquerel and Gaudin have received no satisfactory explanation or useful application.

My own experiments, which are the object of this memoir, seem to prove that M. Ed. Becquerel was mistaken as regards the Daguerreotype plate, in so far as he attributed to the red, orange and yellow glasses a continuing action of the effect of the photogenic rays.

In the Daguerreotype, when we speak of the *photogenic effect*, we cannot understand any other than that which gives to the surface an affinity for mercurial vapour.

In the case of photogenic papers, it is true that the red, orange, and yellow rays render the parts previously affected by the photogenic rays black or of a darker colour. It is the same with the Daguerreotype plate, which after it has been feebly impressed, darkens rapidly to a violet colour under the radiation of a red or yellow glass. This is the only continuing effect I have observed, and this effect is not *continuing* in a Daguerreotype sense, it has no relation to the property of attracting the mercurial vapour; on the contrary, it will be seen from the experiments which I am about to describe, that the radiations of red, orange and yellow glasses entirely destroy this property. There exists then a certain analogy between the action of the

red, orange and yellow glasses upon the photogenic papers and the Daguerreotype plate; and this continuing action is probably due to the distinct photogenic action possessed by these rays, as I am able to prove by facts of a very positive nature.

These two photogenic actions result from two different principles, nevertheless producing similar effects, as to the colour obtained, on the iodide, bromide or chloride of silver, whether it be found isolated, as is the case on the photogenic paper, or it be found in the presence of metallic silver, as happens upon the Daguerreotype plate; but they produce quite an opposite effect upon the silver plate, whatever may be the colour previously given to the surface by these two radiations, endowing it with a property, the one of attracting, the other of repelling the mercurial vapours. We must take care not to confound these two results; we can conceive two different actions giving the same colour to the iodide of silver, and we can also conceive that these two actions may be endowed with contrary properties as regards the fixation of mercurial vapour.

The facts pointed out by M. Gaudin are the results of an action which does not belong to the Daguerreotype, since they are manifested without the aid of mercury; for we must not lose sight of the fact, that the production of the Daguerreotype image is due only to the affinity for mercury of the parts previously affected by the photogenic rays. It does not then follow from the production of an image without mercury, by crystallization or some peculiar arrangement of the molecules, that the red, orange, and yellow rays exert a continuing action analogous to that which determines the fixation of mercurial vapour.

The experiments of Sir J. Herschel, of Dr. Draper, of M. Lerebours, and of Messrs. Foucault and Fizeau, to prove the protective and destructive action of the red rays, were made with the prism.

These philosophers have thus operated with the isolated rays in all their natural purity, and after them it would have been useless to seek to confirm or to contradict experiments so ably conducted and so conclusive.

Sir J. Herschel, in a memoir published in the Philosophical Magazine for February 1842, approves only of experiments made by means of the prism, as they are less subject to error from the foreign rays, which the coloured glasses never entirely exclude. This observation is perfectly just in theory, but in practice, in the particular case of the photogenic power of different rays and of their different actions, it will be found that these phenomena can be studied with greater facility by using coloured glasses, and that the feeble quantity of foreign rays which they admit, far from interfering with the deductions of the experimenter, serve only to confirm and to render them more conclusive. We shall presently see that these foreign rays are completely neutralized in this class of experiments, and it would have been unfortunate not to have added these tests to those of the solar spectrum, since by the aid of coloured glasses I have been enabled, not only to confirm certain properties of the pure spectrum, but also to discover some others which had escaped my predecessors.

Having examined with a prism the light transmitted through the glasses used in these experiments, I found that the red absorbs two-thirds of the prismatic spectrum, from the space covered by the green to the extremity of the violet, leaving the red, orange, and a little yellow, followed by a very slight trace of green. The orange glass gave more yellow, the green being more decided. The light yellow glass intercepted the half of the spectrum; the red was less intense than in the preceding; the yellow occupied two-thirds of its total length, and the green became very distinct; but as far as my sight allowed me to judge, I could not discover any portion of blue in either case: certainly in the spectrum of the red glass there was not the least trace of it.

I will now detail the series of observations I have made upon light transmitted through certain media—the vapours of the atmosphere, and red, orange, and yellow glasses. These experiments have brought forth some results which will I hope contribute to lay the foundation of a more complete theory of the photographic phenomena.

Having noticed, one densely foggy day, that the disc of the sun was of a deep red colour, I directed my apparatus towards it. After ten seconds of exposure I put the prepared plate in the mercury box, and I obtained a round image perfectly black. The sun had produced no photogenic effect. In another experiment I left the plate operating for twenty minutes. The sun had passed over a certain space of the plate, and there resulted an image seven or eight times the sun's diameter in length: it was black throughout, so that it was evident, wherever the red disc of the sun had passed, not only was there a want of photogenic action, but the red rays had destroyed the effect produced previous to the sun's passage. I repeated these experiments during several days successively, operating with a sun of different tints of red and yellow. These different tints produced nearly the same effect: wherever the sun had passed there existed a black band.

I then operated in a different manner: not content with the slow motion of the sun, I moved the camera obscura from right to left, and *vice versâ*, lowering it each time by means of a screw. In this manner the sun passed rapidly over five or six zones of the plate. Its passage was marked by long black bands of the diameter of the sun, whilst the intervals were white. It was then evident that the red and yellow rays, which alone were capable of piercing the fog, had destroyed the action produced by the little photogenic light which came from the zenith.

I then operated with coloured glasses. After exposing a plate covered with a piece of black lace to daylight, I covered one half, and submitted the other to the radiation of a red glass: the mercury developed an image of the lace on the part which had been acted on only by the white light; the other, which had afterwards received the action of the red rays, remained black. The red glass had destroyed the photogenic effect in the same manner as it had been done by the red light of the sun.

I made the same experiment with orange and yellow glasses, and obtained analogous results, but in different times.

Then, having exposed a plate to daylight, I subsequently covered it with a piece of black lace, and exposed it again under a red glass : this produced a negative image. The red had destroyed the effect of the white light in the intervals of the lace, the threads of which preventing the action of the red glass, produced a white image upon a black ground. In operating in this manner upon one-half of the plate, exposing the other half covered only by the same lace to the light of the day, I obtained by the first a negative, and by the second a positive image. The orange and yellow glasses give the same result, paying regard to the difference of time in their respective actions.

All these experiments prove what has been already observed by others before me, but in a different manner, that the red, orange and yellow rays destroy the effect of the photogenic light, whether these rays be produced by the prism or by the action of coloured media ; but, I believe, it has not been observed by any one before me, that after the destruction of the photogenic effect the plate is perfectly restored to its former sensitiveness to white light.

After exposing a plate to daylight, and then submitting it to the destructive action of red, orange or yellow rays, it will be found again sensitive to the same white light.

I have obtained plates which present an equal and uniform image, although the one-half had been exposed to light, and then restored by the red, orange or yellow glass, while the other half had received only the single and final radiation. We may then expose a plate to light, destroy this effect by the action of red or yellow glass, which renders it again sensitive; then expose it again to light, destroy this second effect by the same coloured glass, and so on for many times, without changing the properties of the surface; so that if we stop after any of the exposures to white light, the plate will receive mercury ; but if we stop after any of the exposures to red, orange or yellow light, we shall obtain no fixation of mercurial vapour.

Having exposed a plate to the two actions alternately, first, once upon one zone, twice upon another, and so on until the last zone had been exposed and destroyed six times, I covered the plate with a piece of black lace or an engraving, finally exposing the whole to white light; the result was an equal deposit of mercury upon the whole surface of the plate. The impression of the lace or engraving seemed to be the result of a single exposition to light, as would have been the case with a normal plate; therefore the action of the red, orange, or yellow glass upon a plate previously affected by light, produces the same effect as a fresh exposure to the vapours of iodine or bromine, when we wish to restore the plate to its first sensitiveness.

This restoring property of the coloured glasses may be of great use in the Daguerreotype manipulation. Instead of preparing the plates in the dark, it may be done with impunity in the open light. To give sensitiveness, we have only to place the plate for some minutes under a red glass before putting it in the camera obscura. The frame or box used to hold the plate, if furnished with a red glass at the bottom,

will serve for this restoration. I have obtained in this manner images equal in effect to those produced on plates prepared in the dark.

This possibility of preparing plates in open day offers a great advantage to those who wish to take views or pictures abroad, and who cannot conveniently obtain a dark room. Again, in the case of a plate which has been left too long in the camera obscura, or accidentally exposed to the light, instead of rejecting it, we can restore its sensitiveness by placing it under a red glass. There is still another useful application of this property: if after one or two minutes' exposure to the mercury we perceive the image is too rapidly developing, or presenting signs of solarization, which a practised eye discovers before it is too much advanced, we have only to stop this accumulation of mercury by exposing the plate for a few seconds to the red light, and again place it in the mercury box, to complete the modifications, which give the image all its tones and the most favourable tint. In truth, we may complete all the operations of the Daguerreotype in the open air, in the middle of a field if necessary. We can introduce the plate into the mercury box, in the same manner that we did in the camera obscura, by means of the same frame and red glass, which also serves to protect it when we take it from the mercury to rapidly view its development. I say rapidly, for if we expose it too long to the red light, the photogenic effect will be neutralized. We shall presently see that the time required to observe the state of the image is not sufficient to affect its affinity for mercury, if it be found requisite to replace it in the mercury box. The exposure under red glass necessary to destroy the effect produced by white light, must be a hundred times longer than has been the exposure to white light, that of the orange glass fifty times, and that of the yellow glass only ten times; thus a plate exposed to white light for a second will be restored to its former sensitiveness in ten seconds by the yellow glass, in fifty by the orange, and in a hundred by the red. As soon as the sensitiveness of the plate affected by white light is restored by the coloured glasses, it may be affected again by the photogenic light. It is not even necessary that the restoration should be complete; at each degree of restoration the plate is capable of receiving an accumulation of photogenic effect. If the red rays have not acted more than fifty times longer than the daylight, only half of the effect will be destroyed; if twenty-five times longer, one-fourth; and so on in proportion.

Besides the destructive action of the red, orange and yellow glasses, these same radiations are endowed with a photogenic power, that is to say, they have, like the blue and violet rays, the power of causing the fixation of mercurial vapour. Therefore these radiations are endowed with two contrary actions; the one destructive of the effect of the photogenic light, and the other analogous to the effect of this light.

If the red, orange and yellow radiations of the prism had not also the power of operating photogenically, it might be supposed that this action of the coloured glasses was due to some of the most refrangible rays transmitted by these coloured media. But this cannot be; for if the photogenic action of the red, orange and yel-

low rays were the same as that of the more refrangible rays, it could never develope itself under the destructive action which the same glasses carry with them.

But there is yet more; each ray of the spectrum has its own photogenic action, and they are in this respect independent of each other, and of a different kind; so that the one cannot continue the effect commenced by the other, whether it be for the production or for the destruction of the photogenic effect. I would again observe, whenever I speak of a photogenic effect, I mean that which gives to the Daguerreotype plate the property of attracting the vapours of mercury.

If we expose a plate covered by an engraving to the red light 5000 times longer than is required to produce an effect by white light, we obtain by the fixation of mercury a feeble image, the lights of which are of a grey tone. I could never go beyond this feeble image, which appeared to be the maximum of effect for the red glass. It is impossible to attribute this effect to some feeble quantity of rays, properly called photogenic, passing through the coloured glasses, for we have seen that the blue and violet rays cannot operate under the destructive action of the red rays; this fact proves then evidently, that if the red radiation has a photogenic effect, it cannot be due to the same principle which produces the photogenic effect of the rays situated at the other extremity of the spectrum. The yellow glass has also a peculiar photogenic action of its own, it is a hundred times slower than that of white light, whilst its destructive action is not more than ten times as slow. We can obtain by the photogenic action of the yellow glass an image almost identical, as to force and colour, with an image produced by daylight; with this difference, that the excess of action does not give the blue solarization which we observe upon plates strongly affected by daylight.

The different nature of the photogenic action of red, orange and yellow glasses, from that of the daylight, is also proved by the fact, that the photogenic action produced by these coloured glasses cannot be destroyed by their own reversing action, although the red will destroy the photogenic action of the yellow, and both of these will destroy the action of daylight.

The double property of producing and destroying a photogenic effect is manifested upon a specimen which offers on one-half of the plate a negative image, and upon the other half a positive image, produced at the same time by the same radiation. The length of time necessary to operate with the red glass has not allowed me to obtain a good impression, but I have succeeded perfectly with the yellow glass. The experiment is especially beautiful, and has been thus made:—

I exposed one-half of the plate to daylight for one second, keeping the other half in the dark. The entire plate was then covered with an engraving and exposed under a light yellow glass during ten seconds for the part previously affected by white light, and during a hundred seconds for that which had been kept in the dark. The yellow glass destroyed on the first half the effect of the daylight wherever the plate was not protected by the black lines of the engraving, and the parts only which

under these lines had been protected from the destructive action, received the mercury, producing a negative image; while the same radiation of the yellow glass had operated photogenically upon the other half, developing a positive image by the fixation of mercury upon the parts corresponding to the lights of the engraving.

Having exposed a plate with an engraving under the red glass for sixty minutes, I replaced the red by a yellow glass, without the engraving; after exposing the half of this plate for five minutes under this yellow glass, the other half being kept in the dark, the mercury produced a negative image on the half exposed to yellow light, while the other gave no trace of either positive or negative action. This result can only be explained in the following manner :—

First. That sixty seconds had not sufficed for the apparent action of the red upon the half not exposed to the following radiation of the yellow glass.

Secondly. That nevertheless there had been the commencement of an action upon which the yellow glass had to exercise its destructive action.

Thirdly. That while the yellow glass was occupied in destroying the photogenic action of the red glass, restoring the surface to its primitive state, it was exercising a photogenic action upon the parts protected by the engraving from the red rays, and in five minutes this photogenic action of the yellow glass had produced a negative image by operating upon the shadows of the drawing.

It results from the experiments I have described, that the solar radiation, when modified by coloured media, is in the Daguerreotype process endowed with several different photogenic actions, corresponding with various rays of the spectrum.

The various photogenic actions of the modified solar radiation have distinct characters; each of these modifications is endowed with a photogenic power peculiar to itself, and which gives an affinity for mercurial vapour to the Daguerreotype plate. These various actions are so different, that we cannot mix them artificially to assist each other, as they are antagonistic. The effect commenced by the blue rays is destroyed by the red and yellow; that which was produced by the red is destroyed by the yellow; the effect of the yellow rays is destroyed by the red; and the effect of the two latter is destroyed by the blue; each radiation destroys the effect of the others. Thus it appears that each radiation changes the state of the surface, and each change produces the sensitiveness to mercurial vapour when it does not exist, and destroys this sensitiveness when it does exist.

The alternate change of the state of the plate by these various radiations seems to prove that the chemical compound remains always the same under these different influences; that there is no separation or disengagement of the constituent elements.

If the blue radiation or white light liberates iodine or bromine, these elements would evaporate or combine with the silver surface immediately beneath. If we take the first idea, how comes it that the red radiation re-establishes the compound in its primitive proportions; and, in the second case, how does it happen that these rays are capable of decomposing the surface beneath, liberating the iodine or bromine, and

then combining them again with the upper surface? It is impossible to admit that the red radiation is endowed at the same time with the property of separating and the property of reuniting the same elements. We must then attribute it to a particular force—electricity perhaps, which might accompany each radiation, and which, under the influence of the one, would act positively, and negatively under the other, without changing the chemical compound. In one case this influence would give the affinity for mercury, and in the other destroy it.

At all events, we must look for another explanation of the phenomenon than the one which has hitherto been received, viz. the decomposition of the iodide of silver by the action of light. It is true that light decomposes iodide of silver, forming a subiodide, but this seems to require a longer time than that during which the surface is endowed with the property of attracting the vapours of mercury. In fact, the last property is communicated nearly instantaneously, which is not the case for the decomposition of the iodide by the action of light.

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